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BRAITHWAITE'S RETROSPECT.

VOL. XLII. JULY—DECEMBER, 1860.

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THE

RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M.D.,

LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN AT THE LEEDS SCHOOL OF MEDICINE, ETC.

AND

JAMES BRAITHWAITE, M.B., LOND.

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In this Volume the reader will find a large amount of information which we have collected from various sources within the last six months. We have endeavoured to take the "cream off the bowl," and to throw the old milk away. At the same time we ought to say, that we have been obliged to reject numerous papers of the highest value, but which we did not consider sufficiently practical for our purpose. Passing over, as almost too numerous to notice in this introductory summary, the numerous and highly interesting papers in the body of the following pages on the various subjects in Medicine and Surgery, we have to point out here, many interesting facts and suggestions which have been published within the last six months, on "*Midwifery, and the Diseases of Women.*"

Few special branches of our Profession now engross more of the attention of Medical practitioners than does this subject, and when we include the Diseases peculiar to Children, this branch of Medicine becomes still more comprehensive, and requires still more industry and reading to keep up with the literature of the day. For these reasons we have for some time re-printed, from each of our larger volumes, a pamphlet which the Medical practitioner might easily carry in his pocket, and read at the bed-side, whilst passing some of those wearisome hours which his irksome duties call upon him to spend.

In this little pamphlet the Obstetric practitioner will find most of the facts and ideas which have been published in various papers during the last six months, on the subject of Midwifery and the Diseases of Women. It will thus save him immense trouble and labour in referring to all the published literature of Great Britain, Ireland, and America, and enable him in an hour or two, to make himself master of things which have taken us many weeks, and even months, to collect and condense. Amongst other things, we will first direct the reader's attention to a new pair of Forceps, in great use in America, and especially by Dr. Knight, of Baltimore. We have long thought that the forceps required improvement—that they might be made smaller and to fit the head better, thus running less risk of

lacerating the perineum. These forceps of Dr. Knight, described in the first article on Midwifery, seem to us to deserve the attention of the British practitioner. They are smaller, more easily applied, fit the head better, have no fenestra, and possess other advantages which Dr. Knight describes.

It often happens that the child is low down in the pelvis and all ready for delivery, but the pains abate, the powers of the mother give way, and the practitioner feels convinced that if his fingers were a little longer and a little stronger, he could easily deliver. We want, then, a pair of very small forceps, so as to be useful at the outlet of the pelvis, to fit the head better, and to be able to introduce the upper handle without bringing the patient to the edge of the bed. This last can only be accomplished by making the handle of the upper blade with a *hinge* in it, so as to bend at any angle during its introduction, and keep the handle from the bed; or to make this upper blade in two parts, the lower part or handle to be fixed to the blade when this has been introduced. The parts to be well secured when the forceps have fitted well. The simple introduction of such a pair of blades, with very little traction, would often save the patient and the practitioner hours of pain and suspense. These remarks will apply not only to Dr. Knight's forceps, but also to a good paper by Dr. Doig, of Louth, who has compared the uses of the Forceps and Perforator. Why should we sit days and nights by the bed-side doing nothing, when, by the judicious use of the forceps, we might end the business at once, with safety to both mother and child?

Dr. Braxton Hicks suggests that the old method of Turning the child in the womb may frequently be improved. He describes his method in the following pages: but we confess that we cannot agree with his practice, as there are few or no cases, in which turning is required, where it cannot be done with perfect ease in the usual way, except in cases of impaction low down in the pelvis; and in these last-mentioned cases it can be accomplished with comparative ease, even when the head of the child is very low down indeed, by getting hold of one leg and securing it by means of a bit of strong tape, or the end of a pocket handkerchief. By this contrivance we have great power of traction with the left hand, and can at the same time push up the head with the fingers of the right hand, so that there are no cases which require turning which we cannot treat with safety

and ease. Why, then, have recourse to this external manipulation so well described by Dr. Hicks? Let the reader, however, peruse the paper, and judge for himself. There are some good ideas in it which may require to be adverted to hereafter.

It is becoming a more and more important opinion, first brought prominently before us by our eminent friend, Dr. Simpson, and mentioned at great length in one of our former volumes with several wood-cut illustrations, that there are numerous cases of Deformity of the Pelvis, or unusual size of the child's head (in which hitherto labour has been obliged to be accomplished by instrumental aid, either by the forceps or by craniotomy,) which can be terminated safely both to mother and child by means of turning. Dr. Robert Barnes's paper in this volume illustrates the truth of this opinion well, and the operation of turning in the early stages of labour, when the os uteri is well dilated or very dilatable, is really so easy, especially since we can dilate the parts still more by the use of chloroform, that we shall not be sorry to see this practice still more carried out in cases of difficulty and tardiness, where either mother or child are in danger. Dr. Barnes truly says, that in certain cases the forceps may be used with great success, but in other cases "this instrument will not effect delivery, obstructed through disproportion, with less risk to the mother or with more safety to the child than can be attained by turning." By getting hold of the leg or legs and turning, we have in fact a degree of power given to us which we can exercise with great safety and with wonderful success.

Puerperal Convulsions are often of the most alarming description. We are often unable to administer any remedy by the mouth; a single drop of croton oil on the tongue moistened with a little water is, perhaps, the only thing we can do when the convulsions are incessant. Dr. Scanzoni, of Wurtzburg, suggests that we should inject subcutaneously a solution of meconate of morphia, in the manner lately recommended by Dr. Wood, of Edinburgh, in other cases. Of all the preparations of opium we have found none so good as the *Meconate of Morphia*. Practitioners will find it to be followed by very few of the bad effects of opium. At the same time we may here say that, next to the delivery of the woman, we have found no remedy to exceed in value the injection of an enema composed of half an ounce of spirit of turpentine, well mixed with the yolk of an egg and

thin water gruel; this to be repeated, if necessary, every two, three, or four hours. We have repeatedly known this instantly to stop the most violent convulsions. We even venture to think that, in most convulsions dependent on some *excentric* cause (either in the abdomen or pelvis), whether in women or children, this remedy is superior to all others, especially in children when the convulsions are owing to some intestinal cause. In this case a teaspoonful or small dessert-spoonful of turpentine will be enough.

Mr. Spencer Wells records some good cases of Ovariotomy, and warns us against the too common exhibition of opium in the after-treatment. He says that opium, and not the operation itself, is sometimes the cause of death; and in other cases *stimulants*, too incautiously given, have done as much harm as opium. At the same time starvation and purgatives may be equally objectionable. One great secret of success in the after-treatment of these cases is *perfect rest*, in a well-ventilated room.

An interesting question has arisen lately, respecting the Diagnosis of those cases which have been called Pregnancy, in which the fœtus has seemed to be absorbed, or in some way to disappear. Dr. James Jones, of the Metropolitan Free Hospital, gives us a case of this kind. The menses disappeared—the movements of the child were evident—the abdomen enlarged and the breasts increased—and there were all the most important symptoms of pregnancy. In nine months labour seemed to commence, but disappeared; and in eighteen months the abdomen had lessened and was occupied by a firm mass, more like a fibrous tumour than a child. Such, in fact, would naturally be the opinion of most practitioners when the diagnosis could be thus apparently verified after so long a period had elapsed after parturition was due. But we do not take this view of such a case. We have ourselves lately had a case very similar to the above, and which has ended in the same fibrous enlargement of the womb. But in our case this additional symptom of pregnancy was present—the fœtal heart could be distinctly heard, both by the senior and junior editor of this work, and not once or twice, but repeatedly: this fœtal pulsation was too well-marked to be mistaken—and yet this patient had all the most important symptoms of pregnancy with the most energetic movements of the child. All this has now disappeared, the patient is much improved in health, the abdomen has considerably subsided,

leaving a hard fibrous enlargement of the womb, but no labour has come on, and no child has been expelled. One curious feature of this case was, that we could never find the os uteri, which seemed completely occluded. As far as the finger could reach in all directions, especially backwards, where we might have expected to find it, we carefully sought for this orifice, but never found it, and up to our latest examination we failed to do so.

With this case in our own practice we were the more interested with the case of Dr. E. J. Fountain, of Davenport, Iowa, which resembles our own in many particulars, except that he had the advantage, by the ultimate expulsion of the fœtus itself, of proving the correctness of his own diagnosis. The case will be seen related at page 352 of this volume. This woman became pregnant about June, and continued to enlarge till December. From this time the enlargement ceased to increase, and gradually decreased. The movements of the child were evident, and the usual symptoms of pregnancy present—yet at the end of eleven months no labour had come on, and Dr. Fountain thought fit to bring it on artificially, when there was expelled "the remains of a fœtus, enveloped in its membranes unbroken, with the placenta attached," and not larger than a good-sized orange. It was greatly compressed and emaciated, the flesh had been almost entirely absorbed, leaving little more than the skeleton partially covered with a thin remnant of muscular tissue. From these cases, therefore, it seems possible for a woman to be pregnant, and eventually for the fœtus to be more or less, if not entirely absorbed, or to disappear so far as not to interfere with the functions of health.

Mr. Erichsen has given us some valuable hints about Tumours of the Breast, particularly those small cystic tumours, with a considerable degree of hardness, which are not unfrequently mistaken for cancer by the most experienced surgeons. The great thing to observe is to try to find out whether there is a degree of *elasticity* about them, or whether they are *obscurely elastic on deep pressure*. If so, introduce an exploring *trocar* (*not a grooved needle*). Even in cancer, this trocar can do no harm, and if it be a cystic tumour the fluid will ooze out and settle the diagnosis. The trocar must be pushed *directly* into the tumour and not sideways, else the skin will form a valve, and only blood will escape, thus confusing the diagnosis. The trocar alone will often cure

the cyst; if not, you may inject it with iodine, or cut a bit of the cyst out, or introduce a seton. All these things are well known and often practised, but we are surprised not to find mentioned what we have before recommended in similar circumstances, viz.:—the introduction of a good large silver probe, well charged with nitrate of silver, which we have found most useful. The art consists in changing the probe, the principle of which may be carried out either on small probes for cysts in the eyelid, or on larger probes for cysts any where else. You may either melt a little nitrate of silver in a silver spoon and dip the probe in the fluid, coating the end with it *thick* or *thin;* or you may make the probe itself hot (if in a hurry) and it will adhere at once to the required size of the nitrate, which can be warmed and made to fit the probe better by being applied near a gas-light. In treating cysts, then, make a puncture with a bistoury (and this can be done directly or valvularly), and after evacuating it, immediately introduce the charged probe and *poke* it about the cavity. Cystic tumours of the breast are sometimes so mixed up or surrounded by condensed fibrous matter, that the diagnosis may be very obscure; they may even degenerate into cancer.

In the diseases of women, let us always remember that there is the greatest connection or sympathy between the symptoms of disease in the Rectum and disease in the Womb. You may at first think that certain symptoms are uterine when they are owing to disease in the rectum; this can easily be shown by an examination. When, therefore, you cannot find sufficient evidence of disease in the womb itself to account for certain symptoms, examine the rectum. Mr. I. Baker Brown, with his usual practical skill, has pointed this out clearly. In many cases there will be found a *fissure* just within the sphincter, (the same as in males), and *opposite* this fissure a *small pendulous polypoid body*, which will often drop into the fissure like a pea. In this case divide the fissure by Copeland's blunt-pointed straight bistoury, tie or strangulate the polypus, or twist it off, and *plug the rectum well with lint soaked in sweet oil.* Do not disturb the plug for forty-eight hours, after which time there need be no fear of union by the first intention. Let us advise our younger brethren to be attentive to such apparently *little* things, which are daily occurring in practice, while the more important operations happen only a few times in one's life.

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INDEX.

A SYNOPSIS,

CONTAINING A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THE FOLLOWING PAGES: SHOWING, AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE LAST HALF-YEAR. (ARRANGED ALPHABETICALLY.)

AFFECTIONS OF THE SYSTEM GENERALLY.

AGUE.—M. Eissen states that quinic ether, when inhaled during a paroxysm of ague, arrests the attack, and prevents the recurrence of future attacks. This, however, is open to doubt, as the ether only contains kinic acid, which is known not to possess the tonic and antiperiodic properties of quinine. (p. 13.)

BRONCHOCELE.—The author considers this disease as the result of a perverted action of the nervous functions, at least in this country. Acting on this theory, he administers strychnia in these cases, giving at first a dose of the sixteenth of a grain three times a day: if accompanied by anæmia, he combines iron with the strychnia in solution. This treatment, from the cases reported, seems to be followed by good results; the enlarged organ shrinks, the improvement commencing after taking the remedy a few weeks. (Dr. Murney, of Dublin, p. 371.)

CHRONIC RHEUMATISM.—*Congelation.*—It is well known that in many cases the whole list of drugs, likely to be of any use in cases of subacute and chronic rheumatic inflammation of joints, may be gone through, without any good effect whatever. Dr. Arnott affirms that extensive experience in the employment of congelation in these cases, shows that cold thus employed is capable, in the great majority of instances, of immediately and permanently relieving the pain, and subduing the inflammatory condition of the part to which it is applied. Of course, this agent can have no influence on any constitutional affection coexisting with that which is local. The mode of employing congelation is as follows:—By means of a flat iron, break into a fine powder half or three-quarters of a pound of ice, enclosed in a canvas bag, then rapidly mix this with half its weight of common salt, pour the mixture into a piece of gauze, and apply to the part for about six minutes. (Dr. J. Arnott, p. 14.)

TYPHUS FEVER.—The cardiac sounds are said by Dr. Stokes to form the only certain guide for the proper administration of wine in fever. This is a practical rule, and whenever we find the systolic or first

sound so weak as scarcely to be distinguished from the second, and the pulse at the wrist soft, compressible, and rapid, wine or brandy must be administered freely. It is especially necessary to enjoin that nourishment and wine be given at proper intervals during the night, when the depression is usually most marked, and that the patient be not allowed to sleep too long without being awakened for the purpose of taking his wine. Many a patient has thus been allowed to pass into hopeless collapse. It is very common, in cases of typhus, for the patient to be restless and wakeful: do not hesitate to give opiates freely if necessary. (Dr. A. Tweedie, p. 7.)

Typhoid Fever.—*Emetics.*—You will in some cases of typhoid fever positively succeed in cutting short the malady by the early administration of emetics. Ipecacuan is most suited to these cases, and the wine, in doses of an ounce or two ounces, may be given once or twice a day, plentifully diluted with warm water. This treatment is only suitable for the first four days, dated from the inaugural rigors. During this period there seems in many or most cases to be a natural determination of the fever poison to the gastric mucous membrane. (Dr. Brinton, p. 11.)

Typhus and Typhoid Fever.—Contrary to the opinion of some of our best writers, Dr. Kennedy thinks that these are the result of a common poison. In cases of typhoid, the general impression is that, when stimulants are required, it matters little what kind is employed—hence brandy, wine, beef-tea, &c., are often given to the same patient, and often on the same day. This is not good practice: beef-tea and wine ought not to be used indiscriminately; their effects are very different. Wine may be given in *typhoid* much more safely than beef-tea, which often causes secondary inflammation. The nitrogenous material in beef-tea is much greater than in wine. On the other hand, the latest analysis of wine (by Mulder) shows that it contains very small quantities of albumen, iron, &c. Hence the opinion of Cheyne and Barker, advanced forty years ago, appears to be correct, that whiskey and pure alcohol will not supply the place of wine. Carbonate of ammonia is not suitable to cases of typhoid; it causes purging. Dilute sulphuric acid and water, with three or four drops of laudanum in each dose, will often be very useful, for example: ℞. Acid. sulph. dil. ℥ij.; aquæ ℔ss; tinct. opii guttæ 30. Ft. mist., capiat ℥j. tertia vel quarta quaque hora. (Dr. H. Kennedy, p. 1.)

AFFECTIONS OF THE NERVOUS SYSTEM.

Chorea.—Faradization of the skin in cases of chorea occasions a very rapid and marked diminution of the movements, and frequently effects a prompt cure of the malady. It should be applied chiefly to the parts most convulsed, every day, or every other day. (M. Briquet, p. 77.)

DELIRIUM TREMENS.—Give *half-an-ounce* of the tincture of digitalis in a little water. This will generally suffice, but a second dose of half-an-ounce may be required in four hours, and even a third dose. The action is on the brain and not on the heart; the pulse is not lowered but becomes fuller and stronger, and more regular. Smaller doses do more harm than good: doses of half-a-drachm or a drachm do no good, but harm—the pulse becomes intermitting instead of full. This practice of Mr. Jones is a bold one, but his respectability and knowledge deserve confidence. (Mr. G. M. Jones, p. 61.)

Mr. Jones is supported by Dr. A. Wynn Williams, who has also given half-ounce doses of tincture of digitalis in cases where ther was great excitement of the nervous centre. He also has given it to avert *epilepsy*, and recommends it to be tried in *puerperal convulsions*. One or two good doses are less hurtful than the continued use of small doses. (p. 65.)

Dr. Ballard thinks that digitalis is a real counter-poison to alcohol, as belladonna is to opium, and *vice versa*. (p. 63.)

Mr. Wood, of Heene, gives a teaspoonful of the tincture of digitalis every hour for three or four hours, and then half this dose every few hours. (p. 65.)

When arising from hard and recent drinking, leave off the stimulants and do not give opium; treat the case more as inflammatory and congestive. When arising in cases long addicted to drinking, and when from some cause the drink has been left off, you may cautiously give opium, after moderate purgation; and should this not succeed, give "a hair of the dog that bit him," in the shape of porter or brandy and water; a little wine and broth may also be judiciously given. (Dr. J. A. Marston, p. 52.)

HEADACHE, *Congestive*.—In severe headache, owing apparently to congestion, give the usual doses of the extract of belladonna. This drug *excites* the ganglionic nerves, *tones* and *strengthens* them, but depresses the cerebro-spinal system. Hence its value in congestive headache. (Dr. R. Hughes, p. 398.)

[We should rather doubt this being the true explanation. There is just as much reason to think that the belladonna is valuable as a *sedative* to both the ganglionic and cerebro-spinal nerves.—Ed.]

NEURALGIA.—*Hypodermic Injection of Morphia*.—The hypodermic injection of morphia is of no use whatever in cases where active inflammatory disease (caries of bone) produces the pain. It was of temporary benefit in a case of severe pain originating in inflammation of the nerve itself. The cases best adapted to this mode of treatment are those where the pain is of a purely neuralgic character, depending upon general anæmia. (Dr. J. K. Barton, p. 42.)

Obstinate Neuralgia.—Electricity.—It is well known that if an intense current of electricity be applied in the course of a nerve, its sensory power is benumbed for a time, and the same result follows if the nerve is the seat of neuralgic pain. After a time the pain returns, but on repeating the electrisation the recurrence takes place each time at longer and longer intervals, until finally a cure is effected. Sciatica, lumbar, intercostal, crural, and trifacial neuralgia may be thus treated successfully. Nineteen cases of sciatica, many of them old standing and obstinate cases, have been cured by this plan of treatment. (M. Becquerel, p. 47.)

Reflex Paraplegia.—Dr. Brown-Sequard relates a number of interesting cases, showing that paraplegia is frequently produced by irritations starting from very different parts, as the various abdominal viscera, the skin, mucous membranes, and the trunks of nerves. That these irritations are really the cause of the accompanying paraplegia is proved by the following facts:—that cure of the paralysis quickly follows the cure of the irritating cause, whether spontaneous or as the effects of treatment, and that no treatment directed solely to the paralysis has the least effect, until the cause of irritation is removed. The principal diseases which are liable to act thus secondarily upon the spinal cord are affections of the womb, urethra, bladder, kidneys, and bowels.

The various kinds of paraplegia may be classed into two general groups: one in which the amount of blood circulating in the spinal cord is too considerable; the other in which the opposite condition exists. Most cases of reflex paraplegia, and those accompanied by or depending on general debility, belong to the latter; those cases depending on inflammation belong to the former class. Of the various remedies used in these diseases, some diminish the quantity of blood in the spinal cord, such as mercury, ergot of rye, and belladonna; others, such as strychnine and brucine, increase that quantity. These remedies are often indiscriminately employed with positive harm to the patient, whereas the two classes of remedies are adapted respectively to the two classes of diseases.

The urine in an ordinary case of paraplegia from myelitis is, as is well known, almost invariably alkaline; but in cases of reflex paraplegia, unless depending on disease of the urinary organs, it is usually acid, as in healthy people. Paraplegia depending upon inflammation of the membranes of the spinal cord will be chiefly distinguished from the reflex paraplegia by a rigid spasm of the muscles of the back, by the intense pain caused by any motion of the lower limbs or of the spine. Hysterical paraplegia is precede by hysterical symptoms, but it is in most cases, if not in all, patho logically similar to a reflex paraplegia, both depending upon, or being excited by some uterine or ovarian lesion. There are many other forms of paraplegia which it is necessary to diagnose from reflex paraplegia.

Having ascertained from what organ starts the nervous influence which causes the reflex paraplegia, it is of the greatest importance to try to prevent or diminish the transmission of any nervous influence from the diseased nerve or organ to the spinal cord. If the bladder be primarily diseased, employ narcotic injections into that organ; if the stomach, small intestines, or kidneys be the seat of disease, give the narcotics by the mouth. By applying great cold to the spine we excite the vessels to contraction, but this contraction is always followed by dilatation, and an actual increase of the amount of blood circulated in the cord; and a temporary congestion of the cord may be produced by the patient lying upon his back with his head and legs on high pillows. Those remedies must be employed which will augment the vital properties of the spinal cord: the food must be very nutritious and easily assimilated, and the paralyzed muscles must be regularly exercised. (Dr. Brown-Sequard, pp. 18—37.)

Paraplegia due to White Softening of the Spinal Cord.—Give 5 grains of iodide of potassium twice a day, mixed with nearly the same dose of sesquicarbonate of ammonia, in a decoction of cinchona bark, or in infusion of calumba or rhubarb. Be sure to take this before food in the morning, and an hour before dinner, to avoid the decomposition of the iodide by the gastric juice and the setting free of the iodine. In some cases we may give strychnine, as where the affection is slight, but strychnine produces congestion of the vessels, and if pushed much might cause rupture, and thus increase the evil. Belladonna, on the other hand, decreases the quantity of blood in the cord, and ought therefore not to be used unless the physician has that object in view. The patient should sleep on his back and live as well as he can. (Dr. Brown-Sequard, p. 36.)

Tetanus.—*Aconite.*—It is worth while to try the effect of aconite in tetanus, as from some cases related its employment seems to have been followed by some amelioration of the symptoms. In one case five minims of the tincture were given every two hours at first, then increased to eight minims. The improvement in the symptoms dated from the employment of the aconite. The patient was well sustained by beef-tea and brandy. This remedy must be administered very cautiously however, as though it certainly is a powerful nervine sedative, it acts much more powerfully on the heart. Death from aconite usually occurs from syncope. It is cumulative in its action. (Ed. of Lancet, p. 41.)

AFFECTIONS OF THE CIRCULATORY ORGANS.

Aneurism.—*Popliteal.*—For a novel method of making pressure in these cases, see Dr. Moffatt's paper at page 199.

Aneurism by Anastomosis.—The means usually adopted in the treatment of aneurism by anastomosis frequently disappoint the operator; especially the ligature of the arteries feeding the part. In all cases when it is possible, excise the part at once, taking care to make the incision only in sound parts. When the disease is situated in the lips, this proceeding may be adopted to the exclusion of all other modes. If this cannot be done, owing to the situation of the tumour, the most successful treatment will be to ligature the supplying arteries, and at the same time to introduce numerous double threads through the substance of the tumour in various directions. Inflammatory exudation into the substance of the tumour will be excited. In some cases it may be well to strangulate the tumour by means of ligatures, and in small tumours astringent injections or caustics may suffice. (Mr. O. Pemberton, p. 201.)

Internal Hemorrhage.—*Larch-Bark.*—Dr. Hardy, of Dublin, last year showed the efficacy of larch-bark in cases of purpura hemorrhagica (see Vol. xxxix, Synopsis p. xviii.) It is a pleasant form of administering a terebinthinate, and is, it seems, not only of use in cases of purpura, but in most cases of internal hemorrhages, or hæmoptysis, hæmaturia, and metrorrhagia. The tincture may now be obtained of most respectable chemists. (Dr. J. M. O'Ferrall, p. 86.)

Pulmonary Hemorrhage.—In the intercurrent hæmoptysis of phthisis, the tincture of larch-bark, in doses of half-a-drachm to a drachm every second or third hour is a very effectual remedy, succeeding in some cases when the usual remedies (ipecacuanha, lead, tannin, and gallic acid) had failed. It is now the remedy principally used in these cases, in the wards of St. Vincent's Hospital. (Dr. J. M. O'Ferrall, p. 86.)

Varicose Veins.—Mr. Startin describes a mode of obliterating varicose veins, similar in principle to Mr. Erichsen's plan, by passing pins beneath the vein, differing however in the manner of effecting the pressure. A long needle, made of ordinary steel wire, ground to a triangular point, and bent into a curved form in its anterior two inches, is passed behind the vein, (which is very easily and safely done owing to its form) and then drawn through, so as to leave half an inch of the posterior end projecting. The anterior portion must now be broken off, leaving a small piece projecting, and over the two ends, a clasp is placed so as effectually to compress the vein. In many cases the wire itself will effect this sufficiently without any clasp. A pair of forceps is also made specially for passing the needle under the vein. These instruments may be obtained of Savigny and Co., but any workman can convert steel wire (sizes 18 to 21) into the required needles. A reference

to the woodcut p. 204 will render the above description clearer. (Mr. J. Startin, p. 203.)

AFFECTIONS OF THE RESPIRATORY ORGANS.

CHRONIC BRONCHITIS.—In chronic bronchitis, attended with much viscid bronchial secretion, give the following pills:—℞. Ammoniaci, gr. ij; pulv. ipecac. gr. ½; morphiæ mur. gr. ⅛; ammoniæ carb. gr. ij; mucilaginis acaciæ, q. s. Misce. Fiat pilula. Coat the pills with a varnish of balsam of tolu dissolved in chloroform, and keep in a bottle. (Dr. J. Williams, p. 118.)

HAY ASTHMA.—*Stramonium.*—The ordinary way of using stramonium by smoking it, is of little value; unless very long continued it will produce little effect. To obtain an immediate effect, the proper and only way is to fill an inverted glass with the fumes of the plant, and inspire the fumes, taking one long and deep inspiration. A momentary sense of suffocation is induced, then copious expectoration of ropy mucus, and immediate relief. If properly used, i. e., brought into absolute contact with the air-cells of the lungs, stramonium is a direct, powerful, and instantaneous expectorant. (Dr. E. Lawford, p. 108.)

HOOPING COUGH.—*Belladonna and Zinc.*—It is a singular fact that children are very tolerant of the action of belladonna. If begun in the smallest doses and given four times daily, it may be gradually increased up to a scruple or half a drachm daily, without the production of any unpleasant symptoms. If this remedy be combined with sulphate of zinc, and the two given in gradually-increasing doses, an attack of hooping-cough may be very much mitigated. Dr. Fuller, from whose paper the above is taken, states that he had obtained "results exceeding his most sanguine expectations." The whoop rarely lasted more than twenty-one days, and in some instances it had subsided in ten days. If the attack is accompanied by much febrile excitement and bronchial irritation, a cough medicine may be given, containing a drachm of antimonial wine and the same quantity of ipecacuanha wine in two ounces of water; and, if necessary, a blister may be applied to the chest. To children under three years of age, a sixth of a grain of extract of belladonna and half a grain of sulphate of zinc four times daily may be given at first. This must be given as soon as the whoop declares itself, and perseveringly administered in all cases. (Dr. Fuller, p. 116.)

LARYNGEAL DISEASES.—*The Laryngoscope.*—By means of a small metallic mirror attached to a slightly flexible handle, a very good view of the interior of the upper part of the larynx may be obtained. A perforated concave mirror (that used with the ophthalmoscope will answer the purpose) is required also. The laryngoscope

being introduced over the tongue, the light of a candle or lamp may be reflected into the larynx by a proper adaptation of the two mirrors, and at the same time, of course, the image of the interior of the larynx will be reflected into the laryngoscope. By means of this instrument, (re-invented by Garcia and Dr. Levin, of Berlin,) we shall probably be able to treat laryngeal diseases with much greater discrimination and success than formerly. (Dr. Czermak, p. 87.)

PARALYSIS OF THE BRONCHIAL MUSCLES.—Spasm of the bronchial muscles is a well recognized pathological condition, of which we constantly hear; but paralysis of the same muscles is never mentioned by authors, and it is ignored by the practical physician. Yet it undoubtedly occurs, generally as a sequel to acute inflammation of the bronchial mucous membrane, in those cases, so often seen, where wheezing respiration and nocturnal dyspnœa are prominent symptoms. In such cases give the extract of nux vomica in half-grain doses, combined with a grain of ipecacuanha three times a day, along with tonic medicines. The effect will often be surprising. (Dr. J. F. Duncan, p. 111.)

AFFECTIONS OF THE DIGESTIVE ORGANS.

DYSENTERY.—*Creosote.*—In the great majority of cases of dysentery occurring in this country, opium, given by mouth and injection, speedily effects a cure. But in some cases it fails; and it is not, moreover, a drug which can be given in large doses for any length of time. In those cases attended by great irritation and by fœtor of the evacuations, the use of creosote is attended by the best possible effect. The creosote should be given in solution, so as to be applied uniformly and certainly to the whole surface of the diseased mucous membrane. It may be dissolved in glycerine in the proportion of twenty minims to an ounce of the solvent. The creosote may be also used as an injection. Under this treatment the stools will be found to lose their offensive character. Creosote was first recommended in these cases by Dr. Wilmot, of Pembury, who, however, administered it in large doses (℥i.) merely suspended in gruel. (Dr. W. T. Gairdner, p. 120.)

FISSURE OF THE RECTUM.—A very large number of fissures of the rectum are produced by little polypoid bodies, on the opposite side of the gut to the fissure, and fitting into the fissure. "They will be found in almost every case if carefully sought for." The polypus must be removed, and the fissure divided freely with the knife. If the oiled lint be left in the wound for forty-eight hours it is unnecessary to repeat the painful process of reintroducing it. (Mr. I. B. Brown, p. 349.)

Fissure of the Anus.—Under the head of fissure of the rectum are included at least three distinct forms of disease. The first is the true fissure of the anus, a tear or crack in the mucous membrane lining the sphincter ani. The second is invariably coexistent with an enlarged and varicose state of the veins about the rectum. It is originally a small slough, which leaves an ulcer, this increases in size and the edges become hard and everted. It is usually situated immediately in front of the coccyx. The third form of ulcer is a rather deep excavation at the upper margin of the sphincter. It appears to be caused by the lodgment of a small portion of hardened fæces, or a small polypus may cause this form of ulceration. In this third form there is constant irritation of the adjacent parts, and a certain amount of pus and blood is passed at each relief of the bowels. (Mr. J. Rouse, p. 217.)

HARE-LIP.—Instead of using the ordinary hare-lip pins and twisted silk, it is a better plan to pass silver wire through the parts, fastening the projecting ends by small perforated splints, precisely as the quill suture is used in operations on the vagina for prolapsus, or ruptured perineum. The edges may be brought together much more accurately, three or four very fine silk ligatures only are required to be inserted along the edges of the wound, very superficially. The chief advantages of this mode of operating are that the scar is less than when the pins are used, and the ulceration caused by the pins is avoided. (Mr. E. Woakes, jun., p. 208.)

Mr. Price, of the Great Northern Hospital, has invented a new form of needle, by which metallic sutures may be inserted with the greatest facility through the lips of the deep-seated wounds made in operations for the cure of cleft palate and vesico-vaginal fistulæ. It will be found described and figured at p. 210. It may be obtained of Matthews, Portugal Street.

HYDATIDS IN THE LIVER.—Dr. Budd proposes that, in a case of hydatid tumour of the liver, the cyst shall be tapped by introducing a fine exploring trocar, withdrawing the fluid by means of an exhausting pump, taking the precaution that not a particle of air shall enter the cyst. An instrument for this purpose is made by Weiss. Insert the trocar downwards, with the outer extremity of the instrument at a higher level than the body, so that as the liquid rises in the canula it will expel the air from it. (Dr. Garrod, p. 122.)

LIVER DISEASES.—The healthy action of the liver-cells consists in the absorption of certain albuminous, fatty, starchy, and saccharine matters. When this becomes impaired the cells assume a fermentative tendency, leading to the same description of decay in the albuminous matter as that which is induced by putrefaction, or by treatment by acids or alkalies. Hence some valuable hints respect-

ing the treatment of liver complaints. In jaundice, give nitric or nitro-hydrochloric acid, or a watery solution of nitrous acid made to resemble weak lemonade. This acid destroys and *lixiviates* the deposits of cholochrome and other amido-acids. It also tones the digestive powers. We still want a medicine to *stay* the abnormal action of the liver cells. Creasote is useful, and especially when the disorder proceeds from the bowels; also charcoal, gallic acid, and iron. Nitric acid is often not borne because it is not sufficiently diluted: the more it is diluted, the quicker it is assimilated. Pay great attention in all liver diseases to the bowels: give mild purgatives, of salts, Pullna water, water containing one per cent. of ordinary chloride of sodium and a little sulphate. All mercurials are *bad*. This, we know, is against all medical opinion, but there is much truth in Dr. Thudichum's assertion. Bile contains a good deal of phosphate of soda. Without this salt the assimilation of fats is imperfect, therefore try phosphate of soda in food with chloride of sodium; half a drachm or one drachm per day of phosphate of soda may be given. (Dr. Thudichum, p. 125.)

LUMBRICI.—*Santonine.*—The success following the use of santonine for the expulsion of lumbrici much depends on the method of administration. When given by itself it is scarcely more effectual than Dr. Watson's treatment by steel and purgatives alternately. From two to eight grains stirred in from a tea to a table-spoonful of castor oil, according to age, should be administered at night, and a dose of senna the following morning. The effect will be almost specific. When children have passed worms already I have never known the santonine and castor oil, when administered together, fail to eject more of them. (Dr. J. Brisbane, p. 121.)

STRANGULATED HERNIA.—*Chloroform.*—Unless chloroform be given to a very free extent it does not produce perfect relaxation of spasmodically contracted muscle, hence in cases of strangulated hernia it must be given so as to produce a state of perfect coma, as indicated by stertor. If this be produced there are but few cases of inguinal hernia which will not yield to efforts at reduction, if the case is seen sufficiently early. Success to the same extent cannot be hoped for in femoral hernia, as statistics show that this form of hernia requires operation 25 per cent. more frequently than inguinal. (Mr. T. Bryant, p. 213.)

When you are refused an operation, and other means of reduction fail, turn the patient as much as possible *topsy turvy*, as follows: pull the patient to the edge of the bed, place the legs over the shoulders of an attendant, and raise the patient as much as possible on his head and shoulders on the bed, thus throwing the intestines back upon the diaphragm, and to some extent necessarily making traction behind and directly *from* the seat of strangulation. [We should like to read more accounts of this kind

of treatment having been tried in such cases before an operation is proceeded with. Where it can be done it certainly seems more reasonable to pull the strangulated intestine away by the weight of the bowels themselves, than to *push* the strangulated portion *back* with the force which has sometimes to be used.—Ed.] (Mr. W. Jessop, p. 212.)

STRICTURE OF THE RECTUM.—*Tubular Bougie.*—The instruments usually employed in the treatment of stricture of the rectum are solid bougies. These are much more useful as hollow tubes, so made as to pass one over another, in the same manner as Wakley's urethra tubes, for not only are they more readily passed, but are less likely to injure the parts. After their introduction the rectum above may be washed out with warm water, and any accumulations so removed without pain to the patient from passage of hardened fæces over the sensitive contracted portion of intestine. (Mr. Tufnell, Dub. Quarterly Journal, Aug. 1860, p. 53.)

AFFECTIONS OF THE BONES AND JOINTS, ETC.

EXCISION OF THE KNEE JOINT.—Excluding malignant diseases, there is scarcely any disease of the knee-joint for which excision may not be practised. Some recent experiments of M. Ollier, of Lyons, prove, that if the periosteum be left entire, in the vicinity of joints, it will reproduce bone. Hence the operation may be done for even very extensive necrosis. Recent experience has tended to show that it is better to remove the patella, still this is a subject for consideration. In very young children, it must be remembered that the growth of the limb will be retarded or arrested by the operation. The great inducement to perform the operation, is the fact that the mortality is less than that attending amputation. (Ed. of Lancet, p. 168.)

OPERATIONS ON THE PELVIC BONES.—In operations in which it is necessary to remove a portion of the pelvic bones, as in excision of the hip, with extensive caries of the acetabulum, the operator need be under no fear of exposing the pelvic cavity, as the fascia, muscles, &c., lining the pelvis, in cases of disease of long-standing, become so infiltrated with plastic matter, that they form an effectual barrier which protects the pelvic cavity. (Mr. J. Erichsen, p. 179.)

LATERAL CURVATURE OF THE SPINE.—The disease commences as a yielding of ligaments, but very soon the bones themselves become altered in form, the bodies and articulating processes of the vertebræ adapting themselves to the curvature. It is obvious that mechanical means alone will suffice to remedy the disease when this has taken place. Steel, and tonic remedies, will not replace a twisted

vertebra. Tamplin's apparatus, after many alterations, is the instrument now commonly used at the Orthopœdic hospital. The great success in its use depends on its exercising a gradual, unremitting, but gentle pressure upon the displaced vertebra. It is found that by the use of this instrument the spine can be again brought to a straight line, and this accomplished, it can be so held by stays not differing in appearance from those usually worn. The apparatus will be found described and figured at p. 191. (Mr. H. Coote.)

LOOSE CARTILAGES IN JOINTS.—Some surgeons remove loose cartilages at once, while others leave them in the areolar tissue. Experience shows that it is better to leave them in the areolar tissue, or under the skin. The operation for their removal is as follows:—Bring the loose cartilage to the skin as much as possible, straighten the leg, then make a *valvular incision* over or near to the cartilage, and extract it by a scoop, next close the wound, and put the limb in a splint. (Prof. Fergusson, p. 187.)

NEW TREPHINE.—Dr. Galt, of Virginia, United States, has invented a new trephine which will readily cut through the cranial bones, but the instant the internal table is divided all action ceases, and it is consequently impossible to wound the membranes. It will be found described at p. 199.

STRUMOUS SYNOVITIS.—There is no doubt but that, in the very first stages of all cases, true inflammation exists, and if seen so early, treatment must be accordingly. But where the disease is established, by far the most successful plan of treatment is by a combination of the use of the actual cautery, followed by pressure. The iron used for the purpose should be that known as Rust's; it is simply a three-sided prism with blunt edges, mounted by one of its oblong facets to a curved handle. When the patient is under chloroform, the iron, heated to a white heat, must be passed slowly and with gentle pressure in lines an inch apart, across the joint, if possible, from above to below. The skin must not be too deeply destroyed; in properly chosen cases the effect of this is marvellous, the narrow charred lines made by the cautery come away between the fourteenth and the twenty-first day; they should then be healed as quickly as possible. These lines are afterwards marked by cicatrices, the contraction of which, if aided by strapping the joint, will much favour resolution. Issues, potassa-fusa, mercurials, blisters, and poultices, all favour the growth of the very tissue upon which the disease depends. (Mr. R. Barwell, p. 182.)

STUMPS, *Dressing of.*—In dressing stumps after amputation bandaging is not necessary; the strips of plaster should not be above half an inch broad at the part which is to be in contact with the wound. The *position* of the strips should be varied occasionally, else the

parts between the plasters become too prominent. (Mr. F. Symonds, p. 192.)

SUTURES.—Which of the two kinds of metallic sutures (the common wire, and the needle or twisted suture) is the best in amputations, &c.? Union will take place more rapidly when the needle (or twisted) suture is used; it is firmer, and holds the edges together better than the other. Before twisting it round the needle, oil the thread well, and keep it oiled every day, with a camel's hair brush. When the time comes for its removal, the needle will be removed much more easily. (Mr. F. Symonds, p. 192.)

AFFECTIONS OF THE URINARY ORGANS.

ABSCESS IN THE PROSTATE GLAND.—Surgeons have generally been afraid of making any communication between the urethra and rectum, as in abscess of the prostate gland, &c. They have often preferred making a deep incision in the perineum. Mr. Meade disproves this necessity; you may make a free incision into the prostate from the rectum, when it is necessary. (Mr. R. H. Meade, p. 223.)

ACUTE ALBUMINURIA.—Dr. Corfe states that he has repeatedly seen the injudicious practice of giving stimulating saline diuretics in the *later* stages of the disease, followed by the voiding of hematine and red corpuscles, and final suppression of urine; whilst in the commencement of the case a discreet use of diuretics will be followed by a complete alleviation of the symptoms. In this stage, after a small bleeding so as to affect the pulse, give half an ounce of fresh infusion of digitalis every hour, for three doses; this dose may be repeated at intervals of twenty-four hours, each time followed by copious draughts of nitre-drink (nitrate of potash dissolved in barley water). If the disease does not yield, the loins may be dry-cupped daily, and elaterium administered. A good form for exhibiting this drastic is the following: fresh extract of elaterium, half a grain; powdered capsicum, two grains; gamboge, three grains; soft extract of jalap, four grains; make two pills. One to be taken at six in the morning, the other at eight; or a grain of the extract of elaterium may be dissolved in an ounce of spirit of nitric ether, and a small spoonful be taken every hour, until the bowels have been well opened: this latter is Dr. Watson's plan. (Dr. G. Corfe, p. 141.)

DIABETIC URINE.—*New Test.*—Add an equal quantity of concentrated sulphuric acid to the urine, and boil. The mixture will froth up, and after the ebullition has ceased the froth will remain permanent. The mixture itself will turn of an inky blackness in colour, and on dilution with water it will be seen that this is owing to the

production of small particles of carbon, which will subside as a precipitate. The only objection to this test is that when albumen is present a dark colour will also be produced, but then the colour is a deep brown in this case, and on the addition of water it becomes lighter, and there is no black precipitate. (Dr. J. M'Ghie, p. 150.)

Incontinence of Urine.—In a boy of fourteen, give one third of a grain of extract of belladonna twice a day in cinnamon water, in those cases where the affection is suspected to arise from weakness or paralysis of the sphincter of the bladder. Belladonna acts by *toning* or exciting the ganglionic system, and depressing the cerebro-spinal system. (Dr. R. Hughes, p. 395.)

Lithotomy.—Professor Smith, of Maryland, describes his operation for lithotomy at page 242. It seems an extremely simple operation, and has been performed with great success. (See an engraving of his instruments at page 243.)

Allarton's Operation.—Large calculi and enlarged prostates are no real objections to this method of operating. It simplifies the proceeding to introduce the finger between the director and the sound. It is generally said to be best in the ordinary operation for stone to have a large external wound. It is not necessary in Allarton's operation, a moderate orifice is best. Altogether this is a most valuable operation; the wound is less, the hemorrhage less, the constitutional disturbance less, and there is no chance of infiltration of urine. (Mr. B. Holt, p. 236.)

Phosphate of Lime in the Urine.—Phosphate of lime is always described by writers as occurring only in the granular form, and along with the ammoniaco-magnesian phosphate, the two possessing a precisely similar pathological import. But phosphate of lime, occurs frequently in the crystalline form, and the crystals present well-marked and highly characteristic forms, whereby their identification by means of the microscope, is rendered easy and certain. The crystals are generally aggregated, forming glomeruli, or rosettes. The figure given by Dr. Golding Bird, in his work on 'Urinary Deposits,' of some "penniform" crystals, which he considered as consisting of a variety of the magnesian phosphate, are undoubtedly phosphate of lime. Other forms also occur. Deposits of phosphate of lime have a much deeper significance than those of the triple phosphate, for the phosphoric acid of this last phosphate, and all the magnesia are derived from *without*, being contained in the various articles consumed as food; while for the phosphate of lime we have in the system—in the teeth and bones, and also in the nitrogenous tissues—sources containing some pounds weight of this phosphate. From direct pathological observation it is found

that this deposit, when it occurs, is very apt to be persistent; and when it has disappeared, to return whenever the health is reduced from any cause. It is generally associated with marked impairment of the health, and this where organic disease does not necessarily exist. Dyspepsia, debility, emaciation, nervous irritability, feeble and slow pulse, are symptoms belonging to these cases. The urine from which phosphate of lime is deposited is usually pale, and the sp. gravity varies. It is generally feebly 'acid, often decidedly so when first voided; it however speedily becomes alkaline. (Dr. A. H. Hassall, p. 162.)

PHYMOSIS, *New Operation for.*—Ricord's operation, which is the one generally practised in this country, is thought by many to require a too extensive removal of the foreskin, and moreover entails a permanent exposure of the glans. M. Ridreau, a French military surgeon, recommends a new mode of operating, by which a covering for the glans is retained, and a much smaller portion of the textures is removed. The plan of proceeding will be found detailed at page 233.

SOLIDS IN THE URINE.—A very easy mode of determining the amount of solids in the urine is the following: Weigh a piece of filtering paper, then wet it in the urine, as far as it will bear without dripping, and weigh it again—the difference between the two will be the amount of urine employed; then carefully dry it and weigh it again—the difference between this and the weight of the paper before wetting will be the amount of solids present. This mode is susceptible of great accuracy, but without due precaution it may lead to very considerable errors, owing to the drying of the paper being either carried too far, or being insufficient. (Dr. W. Seller, p. 161.)

STRICTURES, *Dilatation of.*—The following plan of treating strictures will be found to involve very little loss of time, and to cause but little inconvenience. Obtain a catheter with about the last two inches prolonged into a conical point (see wood-cut, p. 231), pass this instrument at night as far as it will go, (perhaps only the point is admissible through the stricture), and tie it in its place, making it exert a little pressure. By morning it will be found to have dilated the stricture to a very great extent. This may be repeated once a week, each time using a rather larger-sized catheter. The part where the cone commences will vary with the seat of stricture. When the patient is a man engaged deeply in business, it is often of great consequence to avoid confinement and loss of time, and this plan is especially suited for such cases. (Mr. B. Shillitoe, p. 231.)

AFFECTIONS OF THE SKIN, ETC.

ERUPTIONS ABOUT THE ANUS IN CHILDREN.—In that obstinate form of cutaneous ulceration about the anus in children, attended often by tumefaction and deep fissures, apply the ordinary yellow wash, gr. i. hydrarg. bichlorid. c. ℥j. aqua calcis. The disorder will be speedily cured. (Mr. J. Bell, 249.)

FAVUS.—The inveteracy of favus evidently depends, not upon the difficulty of removing its visible traces, but on the complete infiltration of the scalp with the sporules of the fungus; and no cure can be complete which is not carefully pursued until a complete new growth of scarf-skin has been obtained, perfectly free from all traces of the noxious germs. The hair must be kept shaved, and an alternation of oily applications with soap and water be maintained for a very lengthened period. The simple treatment by oil and soap seems quite as effectual as the medicated ointments and lotions so frequently recommended. (Dr. W. T. Gairdner, p. 247.)

PITYRIASIS.—In P. capitis, in which the hair becomes dry and falls off, one of the best applications is undiluted glycerine. The following is a good application to P. rubra and P. simplex: Oil of almonds and glycerine, equal parts, and one-half of oxide of zinc. (p. 249.)

RODENT ULCER.—This is not at all affected by internal medicines. The great object is to remove the whole of the diseased parts, and to procure a soft and supple cicatrix. There must be no tension on the cicatrix, as this will certainly cause a relapse. You may use deep escharotics, such as the chloride of zinc. In some parts, however, as the eyelids, escharotics are inadmissible: here free excision should be used, followed by transplantation of healthy skin from the neighbouring part. (Mr. J. Hutchinson, p. 245.)

SMALL-POX.—*Prevention of Pitting.*—In cases of confluent small-pox, of a typhoid character, simply painting the parts over with a mixture of gutta-percha and collodion, suffices to prevent pitting to any extent. The crust comes off in large flakes, composed of the dried exudations and the covering material, leaving the skin uninjured. But in cases presenting a more sthenic type, the best plan is to cover the face with a linseed poultice, continuing this application to an advanced stage of the disease. This plan excludes the air, moderates local irritation, and prevents the drying and hardening of the scabs. (Dr. Stokes, p. 249.)

AFFECTIONS OF THE EYE AND EAR.

ATROPINE.—*Strength of Solution used.*—When it is wished to dilate the pupil for ophthalmoscopic investigation, the strength of the solution used should not be more than half a grain of the sulphate to

an ounce of water. This will suffice for the purpose, and the unpleasant effects of a stronger solution will be avoided. (Mr. H. Walton, p. 288.)

CATARACT, *Ophthalmoscopic Investigation of.*—Do not use too strong a light in cases where there is a suspicion of cataract. If the eye be over-illuminated, the striæ in the lens are not seen. (Mr. H. Walton, p. 289.)

CORELYSIS, *or Freeing the Pupil from Adhesions, by Operation.*—As to the prevention of adhesions in cases of iritis, there is no improvement on the old plan by belladonna and mercury. The latter should be administered in frequent small doses, continued some time after the disappearance of inflammation, and no case of iritis should be treated without the use of belladonna. If the iris is not perfectly free at the subsidence of the inflammation, sometimes atropine will cause its separation; but if this does not suffice, the band of adhesion ought certainly to be divided. When the drug is applied to the skin, belladonna itself should be used, rubbed down with glycerine. Atropia is adapted better for application to the eye itself: a quarter of a grain of the sulphate of atropia to an ounce of water, will produce full dilatation of a healthy pupil. A stronger solution acts more rapidly, and its effects last longer, so that if time be very important it may be employed. Pupillary adhesions may actually induce a second attack of iritis, and may moreover protract an established case indefinitely. There is of course no doubt of the optical superiority of a central pupil, and hence of any operative procedure by which the natural pupil is restored, rather than an artificial one formed. The only objection to an operation of this nature can be, that it is possible to tear the lens capsule, and produce cataract: but, in answer to this, it may be said, that out of fifty cases this has only followed once, in one of the earliest, in which the instrument used was not perfectly rounded and well polished. Corelysis (the meaning of which is *pupil-freeing*) should not be attempted until all ciliary redness and signs of inflammation are past; for though it may prevent a return of iritis, it cannot be proposed as a cure. The instruments necessary for freeing adhesions of the iris will be in the possession of every ophthalmic surgeon, except the spatula hook, which is an instrument devised by Mr. Streatfeild, especially for this operation. The pupil is reached by passing the hook through a puncture in the cornea, the puncture varying in situation according to the situation of the adhesion. The operative procedure will be found described at page 256. If the restoration of the natural pupil can be effected, it is a great improvement on the other alternatives—a motionless pupil, or an artificial one. (Mr. J. F. Streatfeild, pp. 250–262.)

GLAUCOMA.—*Graefe's Operation.*—The leading primary effect of glaucomatous disease is intra-ocular pressure. From this spring a multitude of secondary effects: paralysis of the retina, congestion, inflammatory attacks and their various consequences, and excavation of the optic nerve, all tending to one end—total loss of sight and a spoiling of the tissues of the globe. Hence, in the treatment, all our efforts (failing to cure the cause which produces the intra-ocular pressure) must be directed to obviate the primary effect—the excessive tension. This tension once relieved, the retina recovers itself, more or less, and the circulation tends to its natural equilibrium. The only treatment answering these ends is Graefe's operation, or Iridectomy. It is of the most value when performed early, in acute cases. Temporising measures in such cases, as simple puncture, bleeding, mercury, and colchicum, occasion dangerous delay. In all cases, if any sight be left, it should be performed at once; it will often spare this last remnant of vision, and in many cases permanently. (Mr. W. Bowman, p. 263.)

Mr. Hancock's Operation.—Division of the Ciliary Muscle.—The appearances of the bloodvessels in glaucoma are chiefly due to the constriction exercised by the ciliary muscle: therefore, to relieve the disease, divide the ciliary muscle as follows:—Introduce a Beer's cataract-knife at the outer and lower margin of the cornea, where it joins the sclerotica. The point of the knife is pushed obliquely backwards and downwards, until the fibres of the sclerotica are divided obliquely for rather more than one-eighth of an inch; by this incision the ciliary muscle is divided. (Mr. H. Hancock, p. 270.)

MYOPIA.—*Surgical Treatment.*—Mr. J. V. Solomon has found, that, by dividing in a transverse direction some fibres of the ciliary muscle, or muscle of the lens, the focal range of short-sighted persons, whose corneæ are not conoidal, may be doubled in length. The patient is seated in a chair, and the operator standing behind, fixes the globe with the finger of the left hand, whilst with the right, by means of a cataract-knife, he divides successively the corneo-sclerotic union, the pillars of the iris, and the ciliary muscle. (Mr. J. V. Solomon, p. 296.)

OPACITY OF THE VITREOUS HUMOUR.—Opacities of the vitreous humour may be divided into those which accompany acute diseases and those which occur without any other symptom, or any discernible cause. The latter are by far the most grave, yet many of these cases get quite well of themselves. The cloudiness which accompanies acute inflammations, whether ordinary or specific, generally passes away with the subsidence of the cause. (Mr. H. Walton, p. 290.)

RHEUMATIC OPHTHALMIA.—It may occur that the galvanic current will remove rheumatic pains from the limbs. In such cases it may succeed when the eye is attacked, therefore apply an excited thirty-six link Pulvermacher chain, the positive pole upon the spine, the negative upon the closed eyelid, on going to bed. Let it remain on till morning. (Mr. H. Lobb, p. 297.)

MIDWIFERY, AND THE DISEASES OF WOMEN.

CYSTIC AND ADENOID TUMOURS OF THE BREAST.—*Diagnosis from Cancer.*—The principal point on which to rely in the diagnosis of a cystic mammary tumour from cancer, is the presence or absence of elasticity. A cancerous tumour is entirely devoid of elasticity. Those forms of cystic tumours which consist of a number of small cysts enclosed in a fibrous stroma, do not present this character so distinctly. In all cases where, from this sign, it is suspected that the tumour may be cystic, a fine exploring trocar should be pushed into the tumour:—if the contents be fluid this will be evident; if solid, no harm will be done. It must be remembered that cystic tumours are often associated with cancer. Adenoid, or chronic mammary tumours are of common occurrence, and generally occur before the cancerous age, between the ages of 18 and 25. They are lobulated, and non-adherent to the skin or pectorals. There is no cachexy or glandular enlargement, and in fact they appear to be quite local and benign. (Mr. J. Erichsen, p. 337.)

DEODORIZATION *in Obstetric Medicine.*—The oil of tar and lime have been recommended for this object. These will only *retard* the decomposition of organic matter, while "Condy's Patent health powder" will *decompose* it. This powder can be procured at small cost. (Mr. White, Medical Times and Gazette, Sept. 29, 1860, p. 320.)

FLOODING AFTER DELIVERY.—Inject about half a pint of very cold water (iced if it can be obtained) into the rectum; at the same time give cold brandy and water, with one drachm of Curtis's liquor secalis cornuti in it. (Mr. P. Hubbert, p. 319.)

FORCEPS.—*Dr. Knight's.*—These forceps are a decided improvement of the old instrument. The pelvic curve is less, they have no fenestra, and the blades are smaller and taper more towards the handles. In fact, they *fit the head better* and press less on the external parts when the head is advancing, taking up little more room than the head itself. See engraving and description at page 300. (Dr. S. T. Knight, of Baltimore, p. 299.)

METRORRHAGIA.—In cases of uterine hemorrhage from fibrous tumours, carcinomatous growths, or other causes, the tincture of larch-bark is often a remedy of signal service. It is of similar use in cases of hæmaturia. The dose is from half a drachm to a drachm every three hours. (Dr. J. M. O'Ferrall, p. 86.)

PROFUSE MENSTRUATION.—*Quinine.*—The periodicity of the menstrual process is to be referred for its cause to the ganglionic system of nerves. There is considerable analogy between each individual menstrual flow and an attack of ague. Now, when the process occurs too frequently, every fortnight or three weeks, it may generally be controlled by quinine (unless owing to severe inflammation of the neck of the womb.) The quinine may be given in doses of two or three grains every night or every other night, and if nervous symptoms predominate, it may be combined with sedatives, or if anæmia is present, with iron. (Dr. E. J. Tilt, p. 378.)

PUERPERAL CONVULSIONS.—*Subcutaneous Injection of Morphia.*—Professor Scanzoni, of Wurtzburg, considers that a kind of intoxication, produced by opium, leads with more certainty to a favourable termination than any other means recommended in puerperal eclampsia. But, unfortunately, opium cannot always be administered by the mouth, from the comatose condition of the patient, or the rapid succession of the paroxysms. By injecting morphia subcutaneously, however, this difficulty may be overcome, and the effects will moreover be obtained much more rapidly. In one case treated in this manner, after three injections of meconate of morphia, there occurred only two attacks in nine hours, while previously there had been three attacks in an hour and three-quarters. This mode of treatment requires and demands further trial. (Prof. Scanzoni, p. 317.)

TURNING IN ABNORMAL LABOUR.—*New Method.*—If by any means in our power we place the fœtus in a transverse direction in the uterus, with the abdomen downwards, the knees of the fœtus are opposite the os uteri. Even should the back of the fœtus be downwards the case would be rare in which one side or other will not be the most dependent, and consequently one or other knee be within finger reach of the os. It is then obvious, that if in an abnormal presentation (as the shoulder) the fœtus can be made to assume the transverse direction, the labour is at our command. Again, it is obvious from the oval shape of the uterus, that any contraction of of its walls, or any extraneous force, however applied, as by the hand of the accoucheur, will be sufficient to determine the presentation of either the head or lower extremities. It is tolerably easy (supposing the shoulder presents) to place the fœtus transversely in the uterus, by employing pressure on the fundus externally, and

on the shoulder internally, in opposite directions. The direction in which the pressure is made, and the fœtus made to revolve, will vary according to its original position. When placed transversely in the uterus, as before shown, we have its ultimate presentation at command. A leg may be easily seized if desired, or by a little manipulation the breech may be depressed, and made the presenting part. By this means of operating it is unnecessary to introduce the hand into the uterus, and from this several manifest advantages spring, and several evils are avoided. (Dr. B. Hicks, p. 306.)

VAGINITIS.—*Tannin Ointment.*—In cases of simple vaginitis, or superficial inflammation of the neck of the uterus, an excellent local application is an ointment composed of tannin and lard. This may be smeared over a piece of wadding and introduced through a speculum, a thread being attached to it, so that the patient can remove it herself. This does not supersede the use of constitutional remedies as well. (Dr. Foucher, p. 349.)

VESICO-VAGINAL FISTULÆ.—Mr. Price's new form of needle, by which metallic sutures may be inserted with the greatest facility through the lips of the deep-seated wounds made in the operation for vesico-vaginal fistula will be found described and figured at p. 210. It may be obtained of Matthews, Portugal-street.

MISCELLANEA.

CHLORIDE OF LIME STICKS.—Finely pulverised chloride of lime may be incorporated with gutta-percha softened in boiling alcohol, a warm porcelain mortar being used. The mixture may be rolled rapidly to the diameter of a quill, and kept in a wide-mouthed bottle in powdered lime. These sticks thus remain perfectly hard, and when used the chloride slowly exudes, becoming liquid by the action of the air and the skin. (p. 370.)

OPIUM, *Administration of.*—No drug varies so much in its action on different patients as does opium. In one case, perhaps ten drops of laudanum will produce alarming symptoms, whilst in another, even six drachms will have very little effect. This depends principally upon the previous habits of the patient; if he or she has been in the habit of taking a glass or two of wine daily, a moderate or even large dose, may be safely given; whilst if no wine or other form of alcohol have been habitually taken, a very small dose will be required. When the system is, so to speak, saturated with alcohol, the largest doses are required, as in delirium tremens. (Dr. G. Bodington, p. 409.)

Poisoning.—*Modification of Stomach Pump.*—Instead of using the ordinary stomach pump, whereby fluid is forced into the stomach and then withdrawn, it is a much simpler plan to pass a long tube into the stomach, and pour water down into that viscus, then to depress the upper end of the tube, so as to make a syphon, and the fluid will run out of itself. Of course the tube must be sufficiently long to reach below the epigastrium when the upper end is depressed. This plan was, we believe, first suggested by Dr. Pyemont Smith, of Leeds. Dr. Crook, of Northfleet, in a paper published at p. 417, recommends a similar plan, but one not so simple.

Poisoning by Arsenic.—A mere trace of fatty matter in contact with arsenious acid, is sufficient to reduce its solubility to one-fifteenth or one-twentieth of what it would be, *ceteris paribus*, without the intervention of the fatty principle. Jugglers are able to swallow with impunity, pinches of arsenic or solid lumps of that substance, because they have previously taken the precaution of swallowing milk and fatty substances; they afterwards bring the whole up again by vomiting when the company have retired. In cases of poisoning by solid arsenious acid, instantly give the patient a drink of new milk, or administer any other fatty substance at hand. (M. Blondlot, p. 398.)

Poisoning by Datura.—Not only is opium antagonistic to belladonna, cases of poisoning by the one having been successfully treated by the administration of the other; but, as appears from an interesting case related by Dr. Anderson, of the Bengal army, opium and stramonium are similarly antagonistic. In his case of poisoning by stramonium, fifteen grains of the muriate of morphia were administered in eighteen hours, with marked success. Probably all narcotic poisons with distinctly opposite actions will counteract each other: for example, the distinctly coma-producing narcotic, opium, will be antagonized by any of the poisonous Solanaceæ; whilst in poisoning by strychnine, conium and hemlock will probably be found of use. (Dr. J. Anderson, p. 410.)

Poisoning by Phosphorus.—Detection of.—The smallest trace of phosphorus may be detected by its action on nitrate of silver. A piece of paper moistened in a solution of the latter salt, and suspended over the liquid contained in a closed vessel, will be speedily blackened; if the liquid contain much organic matter, the paper may be moistened with nitro-prussiate of sodium, which will produce a violet colour if phosphoretted hydrogen be present. (M. Scherer, p. 416.)

Poisoning by Strychnia.—Tannin is an excellent antidote to strychnia, its action is purely chemical, an insoluble tannate of strychnia being formed. About twenty-five times the quantity of tannin

relatively to that of the strychnine, is required to neutralize the latter. But, in cases of poisoning, as much of the tannin will be absorbed by the other contents of the stomach, it will be well to administer a much larger quantity. If tannin be not at hand, gall nuts may generally be procured and pulverized. (Prof. Kurzak, p. 413.)

Give sulphate of zinc emetics freely, apply the cold water douche, and assist by chloroform and camphor. (Dr. Part, p. 417.)

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ART. 1.—ON TYPHUS AND TYPHOID FEVER.

By Dr. HENRY KENNEDY, Dublin.

Dr. Henry Kennedy, of Dublin, in a paper published in the 'Edinburgh Medical Journal,' lays down the following propositions as the result of his consideration of the relationship between the various types of fever.

"1. Typhus and typhoid fevers exist in Paris, London, Sweden, parts of America, and Dublin; and relapsing fever in Great Britain and Ireland. 2. In Dublin, other types of fever exist, equally distinct from any of these; of which gastric, remarkable for its great duration, the congestive typhus of Armstrong, the *febris nervosa* of Huxham, and in summer, the inflammatory fevers, may be adduced as examples. 3. These may exist in the same family, and at the same time. 4. When a whole family is attacked at once, some may exhibit spots, and others not. 5. Two crops of eruption, as observed long since by Grant, are not uncommon in the typhus of Dublin; and either may precede the other. 6. One of these may be a bright red, and the other of a much darker hue; and they often co-exist. 7. Petechiæ may exist with typhoid fever; and bright lenticular spots without this fever. 8. Bright lenticular spots may be followed by petechiæ. 9. It would seem as if typhus and typhoid fever could exist in the same patient, and at the same time. 10. Whilst in London intestinal hemorrhage is common in typhoid fever, it is much rarer in Paris and Dublin. 11. In Dublin the same hemorrhage is not uncommon in typhus. 12. Cerebral complication is more common to typhoid fever than is usually taught."

Dr. Kennedy has been led to believe, contrary to the opinion of Dr. Jenner and other pathologists, that typhus and typhoid fevers are the result of a common poison. The variation in the types he is disposed to attribute especially to the influence of temperament, of habits, of constitution, and of the amount of the poison taken into the system. To refute the idea of the less contagious nature of typhoid than of typhus, he refers to the papers of Dr. William Budd, which tend to show the contagious nature of typhoid fever. As to their

non-occurrence in the same locality, an answer is found in the work of Dr. Huss; who states that the two forms often arose under the same circumstances, and in the same house—an intermediate form being sometimes developed.

In speaking of the treatment of typhoid fever, Dr. Kennedy states that the practice in Dublin is to give stimulants, not as a matter of course, but because the particular case requires it. He believes that stimulants are used too indiscriminately.

"It seems," he says, "to be a very general impression, that if stimulants are to be used, it matters little of what kind they are. Hence brandy, wine, beef-tea, &c., are constantly spoken of as being given to the same patient. This is surely not the proper way of dealing with such a matter. With the knowledge we now have of the varied composition of these fluids—for the most of them are fluids—it appears to me to be injudicious to use them as is done at present; and I would particularly notice here the difference between wine and beef-tea. For a long period I have been convinced in my own mind that these two cannot be used indiscriminately; for it has been matter of observation to me that their effects are very different. Of the two, and contrary to what is usually thought, wine may be given with much less risk of unpleasant or injurious effects following, than beef-tea; and when the latter is given indiscreetly, either as to time or quantity, it is very apt to be followed by secondary inflammations, which often cause fever cases to be so precarious in their results. It appears to me that the differences spoken of as resulting from the use of the two fluids are well accounted for by a consideration of the difference of their composition; the amount of nitrogenous material—chiefly in the form of fibrine—being so very much greater in the beef-tea than in the wine. On the other hand, the latest, and, as I believe, the best analysis of wine, that by Mulder, shows us that it contains, but only in the very smallest quantities, several of those matters which are essential to our frames; such as albumen, iron, &c. And this analysis bears out, curiously enough, the remark of two of our ablest physicians on fever—Drs. Cheyne and Barker—made above forty years since; viz., that, after a trial of whiskey, which was used for the sake of lessening expense, they found that it would not supply the place of wine. The point brought under notice, and which I am not aware has been specially alluded to by any writer, would seem to be one of very considerable moment, as bearing directly on the treatment of not only fever, but of other acute diseases. Enough, however, has been advanced to call attention to the subject, and show the discrimination that is to be exercised. Cases, of course, will occur where both classes of stimulants may be used at the same time, and with the greatest advantage. But the point is, that they are not necessarily to be so used, and that mischief may result if a proper judgment be not exercised."

Of carbonate of ammonia, he says that he does not think it suitable for the treatment of fever; nor has he for many years used it in this way.

"I had observed that diarrhœa was exceedingly apt to follow its use. In cases of typhoid fever this would not have been so remarkable; for diarrhœa is there a symptom of the complaint. But it also occurred in cases where there was no irritation in the intestinal canal; as, for instance, erysipelas, pneumonia, and other chest affections. In this way my attention was drawn to the effect of the medicine; and I have now little or no doubt that very great caution should be used when ordering it in fevers, more particularly those of the typhoid type. My own conviction on this point is so strong, that I would not give it at all; and the effects of the medicine on the blood, as shewn in Dr. Richardson's very able work, go far to confirm the opinion advanced."

Dr. Kennedy's treatment of typhoid fever is the following. If there be pain in the right iliac fossa, a few leeches are applied—care being taken that they do not bleed too long; if they be not required, a blister is applied, and after it a warm poultice. As internal medicine, he gives dilute sulphuric acid; prescribing from one to three drachms in an eight ounce mixture with from two to five drops of laudanum in each ounce; an ounce is given at intervals suited to the urgency of the case. In its administration, however, care must be taken that it do not *suddenly* check the diarrhœa. In cases where the diarrhœa suddenly ceases spontaneously, its cessation being attended by dangerous chest or head symptoms, Dr. Kennedy has adopted, he believes with benefit, the plan of keeping open a blistered surface for some days, generally on the chest.—*British Medical Journal, Sept. 15, 1860, p. 726.*

2.—ON THE DISTINCTIONS BETWEEN TYPHUS AND ENTERIC FEVER.

By Dr. W. T. Gairdner, Physician to the Royal Infirmary of Edinburgh.

[The cases mentioned in this paper are four cases of enteric fever which occurred in the fever ward of the hospital a little before this lecture was delivered by Dr. Gairdner.]

Observe, now, that judging from our cases, the characteristic phenomena of this fever are not to be found either in its duration, or in its severity, or in its so-called "typhoid" characters. The presence or absence of delirium, or of stupor, or of dry tongue, affords no ground of distinction. One of our cases had severe and protracted delirium, approaching coma; another had no delirium, but was very deaf and apathetic, with a foul and dry tongue for many days; the other two

had hardly any "typhoid" symptoms at all. The heat of skin was equally variable; in one it was scarcely ever much elevated; in all the others it varied greatly at different periods, as also did the pulse. In not one of the cases was there a rapid and decided crisis; but one began to amend about the eleventh day, another about the twelfth day, while the two remaining cases were excessively protracted, and can hardly be said even now, after nearly seven weeks, to be out of danger; the fever having in one of the two degenerated into a kind of hectic type, exceedingly like that of subacute phthisis—a consequence, let me remark, which is very much more to be dreaded in enteric fever than in any other form of specific fever with which I am acquainted.

Nothing, then, can well be more variable or less characteristic than the general symptoms of this fever. I have seen it resolve itself in ten days, with the symptoms of febricula only, or of a mild remittent fever; I have seen it, on the other hand, last nearly as many weeks, and pass imperceptibly into organic disease. It mimics in turn not only all other fevers, but many other general or local diseases—phthisis, pneumonia, meningitis, perhaps more frequently than most others.

Next, as regards complications, we find that in two of our four cases there was intense deafness; in one, if not in both, with disorganization of the ears. In the same two there was pulmonary complication of considerable intensity, and doubtful issue; in one of them resembling broncho-pneumonia, in the other tubercular disease. The other two cases were almost perfectly free from pulmonary or other severe complication, and but for a little diarrhœa at the commencement might have been said to be free from complication altogether.

But *there was diarrhœa, more or less, in all the four cases.* This, I need hardly say, is the particular symptom from which the name "enteric," as applied to this fever, is derived. Diarrhœa, without known cause, with light-coloured, ochrey, or blood-tinged stools, and with a tympanitic state of the abdomen, gurgling on movement, or tenderness on pressure in the right iliac fossa, are symptoms as nearly characteristic of this fever as any that can be named. And yet I would not have you to trust to these symptoms too much, for some of them are absent in the majority of cases; and even diarrhœa may be very transitory, as it was in two, at least, of our cases (not lasting beyond a few days, or even hours); or it may appear to have been determined by a purgative. Further, there may be no diarrhœa; and even in fatal cases, with great ulceration of the intestines, diarrhœa may not appear till a very late period. Besides, in typhus fever, and, indeed, in all fevers whatever, diarrhœa, as an incidental symptom, is by no means uncommon.

Now observe, further, that *in all our four cases, there was a peculiar eruption on the skin.* This, when present, and in sufficient amount to be identified, is the great criterion of enteric fever, as the

eruption of typhus is also of that disease. I will not dwell on the characters of these eruptions here in the class-room. I will ask you to study them carefully in the wards whenever you have opportunities, and particularly to contrast them with one another. Of the maculated eruption of typhus, you have two excellent examples under observation at present. I will only recall to your attention now the fact, fully demonstrated to you before on many occasions, that in most of our cases of enteric fever the eruption has been very scanty, and that we have not unfrequently hesitated for a day or two to make a diagnosis. Indeed, the first crop of the eruption is rarely quite decisive; but as soon as successive crops, even of two or three spots each, appear, all doubt is removed. But in order to be sure, it is usually requisite in this disease, when the eruption is scanty, to mark each individual spot with ink in such a way as to distinguish accurately the period of its appearance; which precaution against error, accordingly, you saw me take in all these four cases. This in typhus fever it is usually unnecessary to do, and from the number of the spots it would be impossible even if it were necessary.

As regards the characters of these eruptions, I may refer you for details to the excellent descriptions and illustrations of Dr. Jenner in the thirty-third volume of the 'Medico-Chirurgical Transactions,' with which I entirely concur.

As regards the characteristic phenomena of typhus, and particularly the relation of the eruption to these phenomena as we have lately observed them in Edinburgh, I have no time to enlarge at present, and I will therefore take the liberty of referring you to a short paper of mine in the number for July, 1859, of the 'Edinburgh Medical Journal,' on the changes that have occurred in our fevers during the last ten or twelve years. You will there find that typhus, like enteric fever, (though not certainly to the same degree,) is a disease of exceedingly variable severity and duration; that its general febrile characters, though following on the whole a certain type, are by no means to be depended on in individual cases; that its complications are few, and follow no regular rule; and that therefore, still more decidedly than in enteric fever, we have usually to fall back on the eruption for its diagnosis.

You will observe, then, that in the diagnosis of enteric fever, as compared with our old and familiar (but, happily, not *of late* very familiar) typhus, I rely not so much on any of the characters of the fever itself, though these are often very peculiar and distinctive when taken all together; but chiefly, and in some cases exclusively, on the eruption. And the eruption I believe to be characteristic thus far, that when sufficiently abundant and well marked, it shows forth quite decidedly which of the two diseases is present. When scanty in amount, and present only for a short time, it sometimes leaves us in doubt, not only whether the fever present is typhus or enteric, but whether it is an eruptive or specific fever at all. Thus you saw that,

in one of our four cases, I remained in suspense for two or three days, but was afterwards quite decidedly enabled to pronounce that the eruption was that of enteric fever. And as regards typhus, some of you may have observed that, in the two cases now in the ward, I did not for a single instant admit of a doubt. The eruption, being a well-marked one, was, to my mind, a conclusive evidence of the nature of the disease, without a single inquiry as to the symptoms or antecedents of the cases. The same perfectly distinctive character of the eruption, at the first visit, has often occurred to me in the case of enteric fever.

Now I presume you will ask, and you certainly will do well to ask, what proof can be presented that the two eruptions alluded to (admitting them, as I think you will do, to be distinguishable from each other in the great majority of cases) indicate two diseases, and not mere accidental varieties of one disease? I have already fully confessed to you that, unless the eruption makes the distinction, there is no sufficient and constant distinction in the symptoms ascertainable during life. Of course there is the additional *fact* of the ulceration of the Peyerian patches—by far the most noteworthy single fact, pathologically speaking, in the natural history of enteric fever. But it will not do to found on a fact like this in the matter of diagnosis; and, besides, after you have settled that there are cases of fever with ulcerated intestines, you have still to settle whether these cases are different in nature from other cases, in many respects similar, in which the intestines are not ulcerated.

[In the summer of 1859, Dr. Gairdner made a very careful survey of the whole "fever field" of Edinburgh, but without any result giving rise to a suspicion that typhus had given rise to anything but typhus, or enteric fever to anything but enteric fever.]

I beg you to notice very particularly these facts; for not only are they full of instruction as to the danger of associating typhus cases with enteric fever, or indeed with any other fever, in the same ward, but they form the most conclusive of all possible proofs that the two diseases are distinct diseases. Typhus fever is not subject to a relapse; and it is a curious and indisputable law with respect to it, that it almost never attacks again, at least within a period of years, those who have had it before. Yet here we have three members of one family, within a few weeks, seized with two distinct febrile attacks, with two distinct eruptions, the attacks being separated the one from the other by a perfectly distinct convalescence. To make these out to be mere varieties of typhus fever would require the whole laws of that fever, as ascertained by innumerable observations here and elsewhere, to be set at nought in this particular instance.—*Lancet, July* 21, 1860, *p.* 53.

3.—ON THE TREATMENT OF TYPHUS FEVER.

By Dr. ALEXANDER TWEEDIE, F.R.S., Physician to the London Fever Hospital.

In the progress of typhus, generally in the second week, but sometimes earlier, symptoms of exhaustion become evident, and indicate the necessity for stimulants; hence wine, or some form of alcoholic fluid, becomes one of the most important remedies in the treatment. The precise period, however, when they should be given must be regulated, not so much by the duration or stage of the fever, as by individual circumstances. Symptoms of exhaustion or sudden collapse, for example, may supervene at any period, even at the very commencement, and require stimulants to be at once administered, and in quantities which can only be regulated by their effects. The state of the circulation, as indicated by the pulse at the wrist, but still more surely by the sounds of the heart, is the usual practical index to the determination of the question. In a former lecture I drew your attention to an excellent paper on this subject by Dr. Stokes, in which it is shown that the cardiac sounds form the only certain guide for the proper administration of wine in fever. When, therefore, we find the first or systolic sound so weak and abrupt as scarcely to be distinguished from the second, (in extreme cases it is nearly annihilated.) and the pulse at the wrist beating in sympathy with the heart, soft, compressible, and rapid, there must be no hesitation in at once administering powerful stimulants, the effect on the heart's action being the guide as to the amount of either wine or brandy, while medicinal stimuli—ammonia, chloric ether, or other remedies of this class—are given in the intervals. It is especially necessary in such cases to enjoin that in the night, when the depression is generally the most marked, nourishment, and even wine, be given at proper intervals, and not to admit the too common excuse offered by the nurse for withholding them—that the invalid was in such a sound sleep that it was not deemed prudent to interrupt it. Many a patient has thus been allowed to pass into hopeless collapse, when, in all probability, by constant judicious stimulation, recovery might have taken place.

It is always necessary to watch the effects of the first few doses of wine, and if the pulse abates in frequency, becomes soft and fuller, the tongue moist, and the heat of the skin not increased; and, when there has been delirium, if the patient becomes more calm, and has intervals of sleep, we may feel sure that the wine is doing good. On the other hand, if the pulse increase in frequency and strength, the skin become hotter, and the patient restless, flushed, and excited, with throbbing of the temporal and carotid arteries, we may conclude either that wine is not suited to the case, or has been given too early, and should therefore be withdrawn. But, as a general rule, it is perhaps better to give wine a little too early than a little too late, since,

if it appear to disagree, it is easy to suspend its use; but it may be very difficult to restore the vital powers if they have been allowed to remain too long unsupported.

Nor should the wine or brandy be discontinued until convalescence is fairly established; but as the symptoms for which the stimulants have been prescribed disappear, the quantity should be gradually abridged by giving smaller portions and at more distant intervals.

In regard to the amount of wine and alcoholic stimulants that may be administered in typhus, no precise rules can be laid down, as the ever-varying circumstances presented by individual cases can alone determine this. It is prudent to begin with half an ounce or an ounce, and to repeat this amount at longer or shorter intervals, according to the effect produced. From six to twelve ounces may be considered to be an average daily allowance, but sometimes it is necessary to give two or three pints, or even more, in twenty-four hours, and, it is surprising to observe, without the slightest intoxicating effect, even when the patient has been previously unaccustomed to stimulants. Indeed, in low fevers, the exhausted state of the nervous system appears to be an antidote to the effects of stimulants—in short, to create a tolerance of wine and diffusible stimulants.

The wine should always be conjoined with nourishment, in order to assist its due assimilation, though in many cases the digestive powers are so feeble that they are unable to elaborate even the lightest articles of food, and therefore the wine or brandy may be given simply diluted with water.

I have just alluded to the daily quantity of wine that it may be necessary to prescribe in typhus, and stated that no precise rules can be laid down, as the circumstances of each case must determine it. You are doubtless aware that there is a great tendency in the present day to revive the Brownonian system, which flourished for a time in the latter part of the last century, in all acute diseases, including fevers, without regard to individual peculiarities. The doctrine inculcated by some teachers with respect to inflammation is, that this process being a deranged nutrition, involving supply and waste, and the waste being considerable while the inflammatory process lasts, there must be a compensating supply; that as the supplies for the formation of the abnormal products of pus and lymph must be drawn from the blood, or from the tissues, or from both, the vital powers become exhausted, in proportion to the organic disintegration that takes place. Hence it is concluded, that the more the inflammatory process draws upon the blood, the greater will be the exhaustion of vital force, and the consequent effect upon the whole frame.

Upon this physiological theory of the phenomena of inflammation is based the overthrow of established therapeutic principles, on which the treatment has been for ages conducted. But surely even the abettors of this theoretical view must admit that the object of treatment is to anticipate or prevent those so-called destructive processes;

in other words, to promote resolution by all available means. Is this to be accomplished by extravagant doses of wine and brandy, regardless of the ever-varying condition of the sufferer or period of the disease?

Similar reasoning is adduced in regard to the phenomena of fevers, whatever be their type or special circumstances. It is against the indiscriminate employment of stimulants in fever that we protest, being convinced that their proper administration requires as much consideration as is generally bestowed on other measures employed as curative agents.

The enormous quantities of wine and brandy recommended in even the early stage of fevers, whatever be the form, the individual circumstances, or whether there be local affections present, have often surprised me, and inclined me to doubt the accuracy of the statements. I have certainly seen intercurrent inflammations materially aggravated by the injudicious stimulation adopted, and on more than one occasion all the ordinary characters of acute delirium tremens supervene when the unlimited administration of brandy had been left to the discretion of a nurse, who fancied that she was only obeying instructions when she poured down dose after dose of pure brandy. There is surely no practical philosophy in such indiscriminate abuse of a really valuable remedy when given on rational principles; and I deem it the duty of every physician who is convinced of the dangerous tendency of the Brownonian doctrine applied indiscriminately in the treatment of diseases, acute as well as chronic, to express his opinion boldly and decidedly, that the young and inexperienced practitioner may be warned of the dangerous consequences of this recently-revived doctrine.

In regard to the class of wines best suited for fevers, claret often answers well, and may be employed, unless a more powerful stimulant be required. With most patients it suits well to commence with, and mixed with water is a grateful beverage. It has also the advantage of being less stimulating, and more likely to suit in cases in which the employment of wine is doubtful. The stronger wines,—port, sherry, or madeira,—properly diluted, are, however, generally preferred. Brandy is often very useful, especially when a more powerful stimulant is indicated, or when wine is either not relished or disagrees. In hospital practice, for obvious reasons, alcoholic fluids—gin, whiskey, British brandy (which is merely malt spirit disguised by colouring matter)—are generally prescribed.

In the Fever Hospital, Cape wine, of the best quality, and British brandy, are used.

Malt liquors are sometimes employed; but they are suited only to the stage of convalescence. When, however, the patient has a desire for them, from having been previously accustomed to their use as an article of diet, they may be allowed in addition to wine or spirits.

It is necessary, in many cases, to calm the nervous system; and

this brings me to the consideration of the exhibition of narcotics, from the judicious employment of which so much benefit is occasionally derived.

Wakefulness, or want of sleep, so common in typhus, requires the employment of opiates; for nothing is more exhausting than two or three nights passed without sleep. Of the different remedies of this class, experience has led me to give the preference to opium, or its alkaloids; but I generally succeed with a much smaller dose than is usually prescribed,—with, perhaps, ten or twelve drops of Battley's laudanum, or of the liquor morphiæ,—repeating the same quantity every two or three hours until sleep is procured. But we ought to proceed on sure grounds, and if there be undue vascular excitement, indicated by headache, acute delirium, flushing and conjunctival injection, the narcotic must be postponed until the symptoms are subdued. In my last lecture I alluded to the beneficial effects of a combination of tartar emetic and laudanum in the cerebral affections of enteric fever, and need not do more, in the present instance, than state, that it answers equally well under the same circumstances in typhus, at the same time administering, in the severer forms, which so closely resemble delirium tremens, wine or brandy in such quantities and at such intervals as the case requires.

Though opium generally acts better than any other of the narcotic class, it disagrees with some patients; and when such idiosyncrasy occurs, we must give a trial to some other remedy of the narcotic class, such as henbane, extract of poppy, or Hoffmann's anodyne liquor. Some speak highly in favour of the extract of belladonna as a narcotic when the pupil is so contracted as to be thought to contra-indicate the employment of opium; but I have no experience of it. I have often observed, however, that when patients, or those around them, have intimated that opium disagrees, if given by enema, it acts like a charm in soothing the nervous system and promoting sleep, concealing, of course, that opium is administered. Twenty or twenty-five drops of laudanum, added to three or four ounces of any bland fluid, is often sufficient, and is, in many cases, a preferable mode of administering an opiate.

As to the repetition of opiates, I would observe, that when the purpose for which they have been given has been accomplished, the remedy need not be repeated, unless the same circumstances recur. The remedy, however, should always be at hand to be administered if necessary.

In no disease are the beneficial effects of blisters more satisfactory than in these severe cerebral affections. But we must be careful not to apply them if there be subacute inflammation going on. When this is subdued, the blister may be applied to the forehead and the temples, which is preferable to blistering the nape of the neck, in which situation vesication is often attended with much discomfort from the constant friction to which the surface is exposed.

In the pulmonary affections to which I shortly adverted in a former lecture,—whether they assume the form of bronchitis, pneumonia, or, more rarely, of pleurisy,—special local treatment becomes necessary. But it must be remembered that not only is the local malady itself of a low or asthenic character, but the general or constitutional powers are depressed, and, consequently, active remedies not only fail in their object, but tend still further to lower the strength. Counter-irritation of the chest by turpentine stupes, or sinapisms, or the application of a blister, often gives marked relief. The secretion of the pulmonary membrane should at the same time be promoted by antimony or ipecacuan, while the general powers are sustained by wine and nourishment.

The management of the convalescent stage of typhus is not unimportant, though often neglected. Much benefit is derived from various remedies of the tonic class—quinine especially, or some bitter infusion, such as that of cascarilla, calumba, or gentian, to either of which the compound spirit of ammonia may be usefully added. The diet should be light and nutritious, and suited to the digestive powers of the patient, and it is always proper to make careful inquiry into the previous state of the assimilating powers in order that a suitable dietetic plan may be laid down. No general rules can be given, as each case must be regulated by its own peculiarities, and if due care be observed in eliciting these, there will be seldom great difficulty in adapting the details.—*Lancet, June* 16, 1860, *p.* 590.

4.—CLINICAL REMARKS ON THE CURE OF TYPHOID FEVER.

By Dr. BRINTON, Physician to the Royal Free Hospital.

[The use of emetics in the earlier stages of many febrile disorders, is of very old date, but had somewhat gone out of use till revived by Sir Henry Holland, in his classical work 'Medical Notes and Reflections.']

In studying the act of vomiting some ten years ago, I made some experiments, (Lancet, 1853, 'On the Treatment of Fever;' and 'Cyclopædia of Anatomy,' article 'Stomach'), which justified the conclusion, that in the vomiting of certain drugs from the stomach, there was incidentally a specific determination of these drugs to the gastric and adjacent intestinal mucous membrane; by virtue of which afflux the vomiting they provoked had the object and effect of removing them from the system.

Extending this conclusion to the study of typhoid and other fevers, of which then and soon after I was treating numerous (about 100) cases of an almost epidemic character, I was led to recognise, in the earlier symptoms of the malady, a specific determination of the fever-

ous poison to the great alimentary tract of mucous membrane ; and especially to its upper part, as represented by the stomach. And as a corollary to this view, it seemed worth while to try the effect of agents which, like emetics, seemed, from my own researches, to determine an increased afflux of blood to this mucous membrane, as well as an increased secretion, and even excretion from it.

In plain English, however dangerous the facility of such a merely humoral theory in general, there seemed to be special physiological reasons for its validity in this instance. On the other hand, trained by the watching of large epidemics, I believed myself little likely to mistake for fever any febricula or slighter malady ; and could see no possible harm in repeating, artificially, an act which I had felt, in my own person, gave some temporary relief to the terrible symptoms which usher in a typhoid fever.

Without troubling you with the details by which I had slowly to convince myself that this measure was really successful, I venture to sum up their results :—

1. Emetics, early in fever, are most advantageous : with proper precautions, always harmless ; in most instances, extremely beneficial ; in some cases, positively cutting short the malady by a speedy cure.

2. They ought only to be given in an early stage, for which we may generally find an arbitrary limit in the first four days from that of the inaugural rigors, but which is practically better defined by the access and duration of the nausea or vomiting of this epoch.

3. Later than this period, they seem to have much less influence. Indeed, in many cases, it may be doubted whether they have any effect at all. While, on the other hand, the purging they often cause —a purging itself also decidedly useful in the outset of the malady— obscures the symptoms subsequently requiring to be recognised and treated, by its resemblance to that diarrhœa (of intestinal lesion) which systematically occurs in typhoid fever, and to that diarrhœa incidental to typhus.

4. Ipecacuan seems, on all grounds, the best emetic ; and the wine, in ounce or two-ounce doses, the best preparation. Warm water should be plentifully taken to facilitate its action.

5. Once or twice a day for three or four days is the greatest number and frequency of vomitings I have found it advisable to induce.

These are the general claims of the emetic procedure. Let me remind you, however, to check them by some cautions and doubts. It would be wrong to receive and endorse the suggestions of even scientific medicine on terms such as would secure acceptance for the outrageous nonsense of homœopathy or any other quackery.

"*Were these really cases of fever?*" On this point it may be added, to the mere assertions of myself and other eye-witnesses, that not only was no characteristic wanting, but that some of the cases most successfully arrested were examples of unusual severity, occurring in circumstances permitting no doubt; cases from a whole family struck down simultaneously, or acquired by a traceable contagion.

"*Are such cures intrinsically probable?*" When, like Prof. Gullaway's pills, any remedies are asserted to cure *all* diseases with the same rapidity and certainty, the mildest of critics may be pardoned a doubt. And, conversely, the very failures of emetics are confirmations of their efficacy by bringing their action under laws such as few of our best remedies quite interrupt or suspend. Emetics cure in the early, not in the late, stage; cure when they help and imitate, not when they thwart and oppose, the processes of Nature; cure the young more easily than the old; and lastly, reduce the fever far more frequently than they absolutely cut it short, to something which, to vary the proverb, though it "can't be cured," *may* "be endured." Sometimes, indeed, after this imperfect cure, you can trace a kind of dwarfed or rudimentary counterpart of every subsequent stage of the fever; the malady being scarcely shorter than usual in point of absolute time, save in the complete ablation (so to speak) of the whole period of convalescence. The patient lies in bed, with a pulse of 80 or 90; cheerful, but weak; asking to get up, but content to obey the prohibition to do so; requiring some food, but no stimulants, and scarcely more medicine: perhaps getting a little gentle diarrhœa—itself best left without special treatment—just enough to show that he *has* bowels, and that the malady has touched them. Between the fifteenth and twentieth days, however, he quietly rises from his bed, recovered.—*Lancet, April* 21, 1860, *p.* 391.

5.—*On the Treatment of Intermittent Fevers by the Inhalation of Quinic Ether.*—Quinic ether is obtained by distilling alcohol treated with sulphuric acid in presence of the quinate of lime. The product of this distillation is a perfectly limpid, colourless liquid, of an agreeable smell, less volatile than sulphuric ether, but still sufficiently volatile to disappear at ordinary temperatures without leaving any residue. M. Eissen states that when inhaled in the dose of two to three grammes (three grains to three-quarters of a drachm) like chloroform, it arrests a paroxysm of ague which has already commenced, and prevents the recurrence of future attacks. The editor of the 'Union Médicale' doubts, however, whether the beneficial effects are not due to the ether alone, and, in fact, kinic-acid is known not to possess the tonic and antiperiodic properties of quinine.—*British and Foreign Medico-Chirurgical Review, July* 1860, *p.* 280.

6.—ON THE TREATMENT OF CHRONIC RHEUMATIC DISEASES BY CONGELATION.

By Dr. James Arnott.

Among the various purposes served by the new remedial agent, congelation, the most striking, if not the most important, is its use in the various forms of chronic rheumatism. I do not bring it forward as a remedy which may only occasionally be of service, for this may be affirmed of numerous other plans of treatment, but as one which extensive experience of its use has shown to be capable, in the great majority of instances, of immediately and permanently relieving the pain and subduing the inflammatory condition of the part to which it is applied. Indeed so few have been the cases in which considerable advantage has not been obtained from its use, that I have attributed the apparent failure more to error in the diagnosis or to a complication of rheumatism with other affections, than to the inefficacy of the remedy. It is almost unnecessary to add that this agent can have no influence on any constitutional affection co-existing with that which is local; but the existence of such a general affection is only known by the local symptoms, and when these are permanently removed the disease may be said to be cured.

If this (or indeed any disease) can be cured by the application of local measures, instead of those whose agency pervades the system, the local measures should be preferred. Few internal medicines of an efficient character can be long persisted in without incurring some degree of danger; and, after all, though the whole constitution is pervaded by them, their beneficial action may be limited to the seat of disease. A coroner's inquest was lately held at Chatham to investigate the circumstances of a death caused by an overdose of opium, administered for the cure of rheumatism; and two days ago, I was requested to visit a gentleman dying from an attack of bronchitis which owed its fatal severity to the extreme debility caused by a protracted course of active medicine for sciatica. A vulgar and pernicious prejudice against local or external remedies is that they "drive the disease inwards;" but when internal remedies succeed in "driving it out," they too often cause, or (or as in the instance just mentioned) predispose to, a worse disease in its stead.

Of the *modus operandi* of congelation in chronic rheumatism it may be difficult to afford a satisfactory explanation. Some may deny that there is any new or peculiar action exerted by it, contending that it is only the thorough development of the antiphlogistic and narcotic virtues of cold applications; while others may regard it as only an appropriate mode of producing counter-irritation, or as a combination or succession of these two remedial agencies. A rational explanation of the mode in which a remedy operates is always very desirable, but experience is the only real test of its utility. The following case illustrates the mode of using the remedy, and its usual effects.

A woman, between fifty and sixty years of age, employed as a cook in a gentleman's family, was, after an exposure to cold and dampness, affected with pain, swelling, heat, and slight redness of both ankles. She walked with great difficulty, and her sleep was much disturbed by an increase of pain during the night. There was no fever, nor other symptoms of constitutional disturbance. I saw her nine weeks after the commencement of the disease, and learned that the colchicum, iodine, quinine, and other remedies, which she had taken, had proved of no avail. On the contrary, her sufferings had increased, and it was proposed to send her into the country for change of air. On the 26th of February last, recourse was had to congelation. About three-quarters of a pound of ice, enclosed in a small canvas bag, were, by means of a flat iron, broken into a fine powder, and rapidly mixed with about half their weight of common salt. The mixture was then poured into a piece of gauze, and applied to both sides, successively, of each ankle, while the foot rested on the edge of a basin. The gauze bag covered a circular space of skin of between three and four inches diameter, and was kept in contact with it for about six minutes. During the last half of this period, the skin was white, hard, and insensible. When this congelation had ceased, a small quantity of pounded ice was placed across the ankles in order to prevent the smarting which would have otherwise accompanied the returning sensation, and the patient was desired to keep the ice applied for a quarter-of-an-hour, or longer than this, if the smarting should return on its removal.

Circumstances prevented my seeing the patient until the third day afterwards, when she expressed in very energetic terms, her thankfulness for the relief which had been afforded. There had been no return of pain after the congelation, and consequently no interruption of her rest at night. All that she now felt was a sense of stiffness of the joints. There had been heat and tingling of the skin, particularly on the second day after the frigorific had been used, but this she had quickly removed by sponging the part with iced water. As the congelation had not been kept up so long as it sometimes is, there had been no vesication produced, and, consequently, none of that tenderness of the skin which follows vesication. The stiffness and weakness of the joints continued for some time, but not in such a degree as to prevent her walking; and had the disease been of shorter duration, these effects of it probably would not have existed. On the other hand, when the disease has continued for a very long period, and produced organic change in the joint, though all suffering may be removed, the stiffness will probably be permanent.

The congelation did not last four minutes; for being desirous to avoid vesication of the skin, I should have preferred repeating the milder application to causing this annoyance from one of greater duration. In determining the proper period during which the part should be kept congealed, it is necessary to take into the account the

strength of the frigorific mixture employed. A large quantity of well-pounded ice and salt, applied when the materials are acting strongly on each other, will produce a more deeply penetrating and a more lasting cold than a smaller quantity not so well prepared. The only instance which I have known of healing by the first intention being impeded after an operation performed under congelation, proceeded from its having been too long continued when produced by a very powerful frigorific; but even under these circumstances there would probably have been no impediment, had an appropriate mode of dressing the wound been adopted.

To the prevalence of two errors must the imperfection in the treatment of rheumatism be chiefly attributed. One of these is the undue influence which certain theories of the nature of the disease have been permitted to exercise; the other is the false view that has been taken of the disposition which rheumatic inflammation has to extend from one part of the fibrous system to another, or of what has been termed metastasis.

The theory of the nature of acute rheumatism at present in vogue is a plausible one, but it ought not to be relied upon to the degree that would render those lessons of experience which are apparently contradictory to it less impressive. That an acid exists abnormally in the blood, there can, reasoning from chemical analysis alone, be little doubt; but whether this acid be the cause of the disease, or only one of its numerous effects, or what the importance of each of these effects may be, are points to be yet ascertained. What has been termed by the late Dr. Todd the eliminating mode of treatment, and which mainly consists in the administration of large doses of some alkaline substance, accords with this theory; but the well-attested advantages proceeding from what are known as the bark, opium, and lemon-juice modes of treatment are adverse to it. So also, though the supposed common causes of rheumatism, cold and moisture, are in favour of the idea that the emunctories of the skin are closed by their influence, the immediate occurrence of this disease, and of the analogous disease, gout, after sprains, wounds, and urethral irritations, would lead to a different conclusion. One of the most severe attacks which I have met with of rheumatic fever, immediately followed the bite of a horse; and one of the severest attacks of gout was the consequence of sudden dilatation or rupture of stricture of the urethra.

The common opinion respecting the metastasis of these diseases is founded partly on imperfect observation and partly on imperfect theory. Inflammation of the heart sometimes precedes inflammation of the joints in rheumatism; and is so common an event during the continuance of the disease, that Dr. Watson has only known two cases of rheumatic fever occurring previously to puberty in which the heart was not affected. That rheumatic inflammation of the heart often takes place about the same time that inflammation ceases in a

joint, is indisputable; but this coincidence no more shows the connection of cause and effect, than the occasional coincidence of dreams and events shows that dreams are prophetic. Rheumatic inflammation extends to various parts of the fibrous system, just as common inflammation attacks successively various parts of the respiratory mucous system; and, doubtless, the part last affected may act in some degree as a counter-irritant in removing the inflammation previously existing in other parts. If it be true (and the idea is, to a certain degree, supported by the pathological researches of Dr. Garrod) that the articular inflammation is produced by the deposition of a *materies morbi*, it must follow that, as inflammation so produced would impede this secretion or deposition, whatever is calculated to moderate or remove it, will, instead of repelling the poison into the blood, not only promote its deposition in the joints, but, by preventing a febrile disturbance of the system from the violence of the local affection, facilitate its excretion by the skin and kidneys. If we take it for granted that rheumatism is a blood disease, we must admit, with Dr. Graves, that the poison, in certain favourable conditions of the system, may pass off by the emunctories without exciting local disturbance—just as electricity may pass from the clouds to the earth without injury to buildings on its surface.

Before concluding, I may mention another circumstance calculated to oppose the general introduction of congelation in chronic rheumatism; I allude to the imperfect manner in which it has been too often used for antiphlogistic and other purposes. To this cause principally I would attribute the failures in its use mentioned in a work on rheumatism recently published by Dr. Fuller. He states that, out of five cases of lumbago in which he employed congelation, there were two failures. As this differs widely from my own experience, I can only explain the difference by the supposition that the remedy was applied by him in a different manner; and this opinion is confirmed by finding it stated in his book, that congelation is objectionable from the pain produced by it both during and after the application. When properly used, the smarting or tingling caused by it is too little to be complained of, and will certainly never be deemed by the patient a counterbalance to the immediate and permanent relief of the suffering from the disease. Dr. Fuller thinks that these failures of two out of five cases supported his theory, that rheumatism of every description arises from lactic acid in the blood, which must be neutralised or expelled by internal medicine. But surely, instead of the two failures, it would have been more reasonable to have adduced the three successful cases as a corroboration of this theory. What appears a metastasis or shifting of inflammation, probably arises from the poison (assuming the truth of the theory) being carried by the *vis medicatrix* to a new outlet or excreting texture, when the previously excreting texture or former outlet has been obstructed or disturbed by inflammation set up by the excreted matter; and, con-

sequently, what would continue or restore the excretion in a joint already affected by removing this disturbing cause, or, in other words, by arresting the inflammation, would appear well fitted to prevent its extension to other parts. The truth is, however, that there is too great a dissimilarity between the various forms of acute and chronic rheumatism to authorise reliance on reasoning of this description, far less the substitution of such reasoning for the results of experience.—*Med. Times and Gazette, July* 14, 1860, *p.* 28.

DISEASES OF THE NERVOUS SYSTEM.

7.—ON AFFECTIONS OF THE SPINAL CORD.

By Dr. C. E. Brown-Sequard, F.R.S., Physician to the National Hospital for the Paralysed and the Epileptic, &c.

[Dr. Brown-Séquard has published in the Lancet a series of most interesting lectures on this subject, from which we have endeavoured to collect some of the most practical remarks.]

On the Diagnosis and Treatment of the Principal Forms of Reflex Paraplegia.—[The present article treats of reflex paralysis from various causes. A number of cases are first related.]

1. *Cases of Paraplegia due to a Disease of the Uterus.*—In 1855, a young lady consulted me for what she called an extreme weakness, which was really a paraplegia, almost complete at each menstrual period. No diminution of any kind of sensibility; no paralysis of the bladder or the rectum; no symptom of hysteria. Dysmenorrhœa, anteflexion of the uterus, which was extremely sensitive, very large and congested, bearing-down pain, &c. In a few days after the use of a bandage to support the womb, a great amelioration was evident; and, in less than two weeks, the paralysis had entirely ceased. It had lasted six months, and had been treated in vain by strychnia, galvanism, shower-baths, steel, and other tonics.

Lisfranc ('Clinique Chirurgicale de la Pitié,' vol. ii., p. 199, 1842) mentions the case of a lady, completely paralysed of the lower limbs, who was vainly treated, by the most active remedies, for a supposed affection of the spinal cord, and who became better only when treated for a chronic metritis. The progress towards a complete cure, which was obtained after a long period, took place gradually, the change in the paraplegia following that of the inflammation of the womb. Lisfranc mentions another case in which also the gradual amelioration of the paraplegia corresponded with that of the disease of the uterus, and a complete cure was obtained.

An excellent observer, Dr. Nonat, has seen seven or eight cases of paraplegia, depending upon an affection of the womb, in which the paralysis was quickly cured after the cure of the disease of the uterus.

Those cases show that when the uterine affection is limited to one side the paralysis is also limited to the lower limb on the same side. The cases have been published by a pupil of Dr. Nonat, Mr. Esnault, ('Des Paralysies Symptomatiques de la Métrite et du Phlegmon Utérin,' Paris, 1857.)

Mr. Henry Hunt, of Dartmouth, Professor Romberg, Dr. Wolf, of Bonn, Leroy d'Etiolles, jun., and others, have also mentioned cases of paraplegia, following a disease of the womb, and more or less quickly cured after the cure of this disease.

What is the mode of production of paraplegia in those cases? We cannot admit that paralysis was due to a pressure upon the nerves of the lower limbs,—at least in most of those instances,—as the enlargement of the organ was not sufficient to produce such an effect. Besides, in those cases sensibility was but slightly or not at all diminished, which excludes the idea that it was chiefly or solely a pressure on the nerves of the lower limbs that caused the paralysis. We must therefore admit that it is either through a peculiar influence upon the spinal cord, or in consequence of some alteration in the blood, that paraplegia takes place in cases of disease of the womb. This last explanation, of which I would not have spoken if it had not been represented to me as the right one, we must reject, because there is no reason whatever why an alteration in the blood should rather produce a paralysis of the lower limbs than of the other parts of the body. We must conclude, then, that it is through a peculiar agency upon the spinal cord that an affection of the womb produces a paraplegia. What this agency is we will try to show in another lecture.

2. *Cases of Paraplegia due to a Disease of the Urethra.*—Graves ('Clinical Lectures on the Practice of Medicine,' 2nd edit., edited by J. M. Neligan, M.D., 1848, vol. i., p. 554) relates the case of a sailor who was treated by Dr. Hutton for a stricture of the urethra, due to gonorrhœa. There were chills, dysuria, and loss of power in the lower limbs. "A very remarkable amendment took place in his back and lower extremities in a few days after the first introduction of the instrument; in fact, it was almost sudden." The paraplegia was soon cured, together with the disease of the urinary canal. Several cases, more or less similar to this one, are related by Leroy d'Etiolles, jun., and other writers.

3. *Cases of Paraplegia due to Inflammation of the Bladder.*—In a patient whom I saw in one of the wards of the Charité Hospital at Paris, under the care of Rayer, and whose case is published at length by Leroy d'Etiolles, jun., ('Des Paralysies des Membres Inférieurs,' &c., Paris, 1856, pp. 57–59,) the lower limbs became weak after an incontinence of urine, due to a gonorrhœa. Cystitis appeared, soon followed by a notable but incomplete paraplegia. Gradually the paralysis diminished, while the inflammation of the bladder was subsiding, and a complete cure was obtained. Several other cases like this one are recorded by Leroy d'Etiolles, Macario, and others.

4. *Cases of Paraplegia due to Disease of the Prostate.*—A patient consulted Rayer, Magendie, and Leroy d'Etiolles, sen., for an enlargement of the prostate, which was followed by a paraplegia. The swelling proved to be due to an abscess, which opened, and continued for some time to give out pus. When the suppuration stopped, the paraplegia began to diminish, and was quickly cured (Leroy d'Etiolles, jun., *loc. cit.*, p. 83). Other cases, nearly similar to this one, have been observed by Civiale and others.

5. *Cases of Paraplegia due to Nephritis.*—Rayer has published several cases of this kind of paraplegia in his classical work on Diseases of the Kidney ('Traité des Maladies des Reins,' vol. iii.) In one case, weakness appeared in the lower limbs of a patient some time after he was attacked with nephritis. An amelioration soon took place in the nephritis and in the paralysis, under a treatment directed against the former. The patient left the hospital; but a short time after he was suddenly attacked again with acute symptoms of nephritis, and lost the power of standing on his feet. Cupping and diuretics quickly cured the inflammation of the kidney, and in less than a fortnight he was also cured of the paraplegia. It must be remarked how quickly in this case the symptoms of paraplegia appeared and disappeared twice after the production and the cessation of a nephritis.

6. *Cases of Paraplegia due to Enteritis.*—Two cases are recorded by Graves, (*loc. cit.*, vol. i., pp. 547–9,) in which the lower limbs were paralysed after an attack of inflammation of the bowels. There was no numbness, no pain, no formication. In one only of these cases the bladder and rectum were a little paralysed. In both patients there was a marked loss of the muscular sense. They both recovered. Zabriskie mentions a case of cure of paraplegia in three weeks, after the cure of an inflammation of the colon, which seemed to have caused the paralysis ('Gaz. Méd. de Paris,' 1842, p. 296). Several cases of paraplegia, apparently due to dysentery, and cured after the cure of this affection, are also on record. (Macario, 'Gaz. Méd.,' 1858, p. 161.)

To the cases of inflammation of the bowels we may add those of irritation of these organs by worms. Such cases of paraplegia cured immediately, or in a short time, are indeed far from being rare. We will only say that instances of this kind have been observed by Bremser, by Mœnnich ('Biblioth. Méd.,' vol. lxi., p. 269) and by Dr. Calvert Holland ('Edin. Med. and Surg. Journal,' 1845, vol. lxiii. p. 325).

7. *Cases of Paraplegia due to an Affection of the Lungs or the Pleuræ.*—Three cases of this kind are recorded by Macario ('L'Union Méd.,' 1859, p. 276). I have seen a similar case (in 1849) at the Charité Hospital. in Paris. The patient I saw was cured in a few weeks. Two of those seen by Macario were cured, and one died.

8. *Cases of Paraplegia due to Diphtheria.*—Maingault ('De la Paralysie Diphtérique,' Paris, 1860, p. 45) relates the case of a young girl who, after an attack of diphtheria, became completely paraplegic. For a fortnight no change, but from that time gradual recovery; com-

plete cure in three months. Trousseau has seen a similar fact in a child nine years old; and Sellerier has seen paraplegia, following diphtheria, cured in a month, in a gentleman forty years old (Maingault, *loc. cit.*, p. 46). I will try to show, hereafter, that paralysis due to diphtheria ought to be considered in the same light as the cases of paralysis more evidently of a reflex origin.

9. *Cases of Paraplegia due to Teething.*—I have seen a very interesting case of this kind in which the paraplegia, which had appeared at the very beginning of the second dentition, in a boy five years old, increased and decreased alternately at the time of, and after, the cutting of each of three molars. In that case there was a slight contraction in some of the paralysed muscles. Simple hygienic means, shampooing, galvanism, and a decoction of cinchona bark, were employed with success. The child was cured two months after the cutting of all his teeth (except the so-called "wisdom teeth"). Underwood has seen a case of paraplegia appearing after every cutting of a tooth. Fliess and others have already shown the relation between teething and paraplegia, and it would not be difficult to prove that those very able writers (especially Dr. Kennedy and Dr. West) who think that it is chiefly the condition of the bowels in teething that causes paralysis have been misled by the fact that very frequently enteritis precedes paralysis. Usually, enteritis in teething, as well shown by Dr. Gain and Dr. Fraser Campbell, is produced by a reflex action, and paralysis seems to be generated in the same way.

10. *Cases of Paraplegia from Irritation of Nerves of the Skin.*—Amongst these cases are the very numerous ones due to cold and wet. Graves (*loc. cit.*, p. 563) relates the case of a gentleman, fond of shooting, who became paraplegic after having exposed his lower extremities to wet. There was no pain in the back; no tenderness on pressure. He was but incompletely cured. This is the kind of reflex paraplegia which usually is most difficult to cure. However, it may be completely and very rapidly cured, as shown by two cases, one mentioned by Dr. T. Watson in his classical work 'On the Principles and Practice of Physic,' in which case the cure was obtained in a few days, and the other by Dr. W. Moore, of Dublin (The 'Lancet,' vol. ii. 1859, p. 282), in which case the patient, who owed his paraplegia to a long exposure to a heavy rain, was cured after six or seven days of treatment.

Graves (*loc. cit.*, p. 557) gives a case of hemi-paraplegia due to the irritation of cutaneous nerves by erysipelas, occupying the calf and inside of the right leg. The erysipelas yielded to treatment, but for several days the patient was altogether destitute of any power of motion in the affected limb. It is evident (says Graves) that this must have been a reflex paralysis, as the muscles which move the leg on the thigh lay far above the part in which the erysipelatous inflammation existed.

11. *Case of Paraplegia arising from Disease of the Knee-joint.*—I have had under my care a gentleman, who, according to all appear-

ances, became paralysed of the two lower limbs in consequence of the irritation starting from an acute inflammation of the synovial membrane of the left knee-joint. The paralysis at first was limited to the left limb, but soon appeared in the other. The arthritis became chronic, and then it was observed, a great many times, that every increase in the pain, due to this inflammation, was followed by a corresponding increase of the paraplegia, and that the reverse occurred, as regards the paraplegia, when the pain diminished or ceased. There was no special symptom of any affection of the spine or its contents—except, of course, the paralysis, and a notable degree of muscular atrophy. The patient, after five months of treatment, was very much improved, and could walk pretty well, except at times, when there was a return of the pain in the knee.

12. *Cases of Paraplegia due to a Neuralgia.*—Two years ago I saw a case of this kind in consultation with Dr. Gordon, of Boston, U.S. As this is not a case of rapid cure, I will not speak of it now. Several cases of paraplegia, with or without atrophy of the paralysed muscles, are mentioned by Notta, ('Archives de Méd.,' Nov. 1854, p. 556.) All the patients have been cured of the paralysis some time after the cure of the neuralgia.

The above cases, and many others which we could mention, show that the production of paraplegia may be associated with irritations starting from very different parts—such as most of the viscera, the skin, the mucous membranes, and the trunks of nerves. In all these cases we find that paraplegia has followed the outside irritation, which we consider as its cause, and that the cure of this paralysis has been more or less quickly obtained after the supposed outside cause had been suppressed. Besides, in many of these cases we find the supposed effect increasing or decreasing gradually, in correspondence with the outside irritation, and in several instances we find that even without treatment the paralysis disappeared after the cessation of its supposed cause; while, as a general rule, no treatment of the paraplegia seemed to have the least influence so long as the outside irritation was not alleviated, or had not entirely ceased. In some cases we find that paraplegia appears and disappears altogether, twice, or many more times, in correspondence with the renewed production and cessation of the outside irritation. Can there be more decisive proofs that it is the outside irritation starting from some sensitive nerves in various parts of the body which produces the paraplegia?

Those persons who, perhaps, are not struck by the evidence in the foregoing facts and reasonings may argue against the view that paraplegia is the effect of an outside irritation in the cases we have mentioned: 1st, that there is no need of admitting such a view, as other explanations of the various cases can be given; 2ndly, that it is not possible to explain by a reflex action the production of a paralysis.

The first of these objections may in appearance be well grounded, as it may be said—

1. That, in cases of disease of the uterus, this organ produces paralysis by pressing upon the obturator nerve and the sacral plexus.

2. That, in cases of disease of the prostate, the urethra, or the bladder, the urine, not being freely expelled, becomes altered, and some of the altered principles are absorbed and act as poisons, producing paralysis.

3. That, in cases of disease of the kidney, paraplegia is due to uræmic poisoning.

4. That, in cases of teething, of diphtheria, and of disease of the bowels, the lungs, or the pleuræ, paraplegia is due to an alteration in the blood, caused by the disturbance of digestion or respiration.

5. That, in cases of paraplegia due to the action of cold and wet, we ought to place this affection in the category of rheumatismal paralyses.

6. That, in cases of pain in the skin, in joints, or in trunks of nerves, paraplegia has no other relation to this pain than that of being produced together with it by some affection of the spinal cord.

I am ready to admit that in some cases paraplegia is produced in the various ways just related; but in most of the cases in which a paralysis of the lower limbs follows an outside irritation from the womb, the bladder, or most of the other organs, when there is no evidence of an organic affection of the spinal cord or of the surrounding parts, it is not possible to explain the production of the paralysis by a pressure on nerves, an alteration of blood, &c. This I must show, as, lately, Romberg, one of the promoters of the view that paraplegia may be caused by a reflex action, has acknowledged that he was ready to abandon it on account of objections advanced by Hasse and Valentiner.

1. As regards paraplegia depending upon disease of the womb, we will remark that, except in cases of very great enlargement, as sometimes in pregnancy, pressure upon the nerves of the lower limbs cannot be the cause of the paralysis. In the case I have mentioned, of a young lady who was cured by using a bandage, the uterus was certainly not larger than in a woman at the end of the second month of pregnancy.

2. It is possible that in cases where urine accumulates in the bladder, and remains there a long while in an altered condition, some of its principles may be absorbed and act as a poison; but, on the one hand, however possible this is, there is no proof that a paralysis has ever been produced in that way; and, on the other hand, we find that it is with the degree of pain that paraplegia has relations in cases of paralysis of the lower limbs following an affection of the prostate, the urethra, or the bladder, and not with the retention of urine, as there is no change in this kind of paraplegia, whether the bladder is carefully voided or not, at every time there is a certain amount of urine in it, unless the irritation starting from either the bladder itself, or the prostate, or the urethra, be diminished.

3. Certainly, in cases of nephritis, there may be paralysis due to uræmic poisoning; but this kind of poisoning does not manifest itself only by a paralysis. On the contrary, it causes other and most striking symptoms, none of which has existed in the case I have cited, and in other more or less similar cases.

4. I am willing to admit that there is some alteration in the blood in diphtheria, or even in dysentery, in enteritis, &c. I am also willing to admit that this alteration may cause a degree of paralysis, but that a complete paraplegia, without any diminution of voluntary movement elsewhere, may be due to a cause which, as it is supposed to be in the blood, is *circulating everywhere in the body*, I think no one can admit.

5. It is true that cold and wet often cause rheumatism; but when, after an exposure to cold and wet, a man is suddenly seized with paralysis, without having the least symptom of rheumatism, we certainly have no right to place his affection in the vague group of rheumatismal paralyses.

6. No doubt that diseases of the spinal cord and its membranes very often manifest themselves by pains resembling neuralgic pains, and located either in a joint or along the course of a nerve; but besides the fact that, in such cases, paraplegia, when it appears, is accompanied by symptoms indicating a disease in the spinal cord or its membranes, which was not what occurred in the cases I have mentioned, there were real inflammations (of a joint, or of the skin) in two of those cases, instead of the *referred* pain due to a centric cause.

We think we may safely conclude, from this examination of the above explanations, that they can be considered as valuable only for a small number of those cases in which paraplegia has appeared after an irritation in some sensitive nerve of the skin, the mucous membranes, and various organs. On the one hand, therefore, most of those cases cannot receive these explanations; and, on the other hand, I have already shown that the period of apparition, the variations of intensity, the progress, and the cure of paraplegia, in the cases I have mentioned, are in perfect correspondence with the view that paralysis depended upon an irritation from a sensitive nerve.

I have now to examine the objection that it is impossible to understand how a paraplegia can be caused by a reflex action. It is, indeed, very easy to show how a paralysis can take place by a reflex influence through an irritation starting from a sensitive nerve. There are two modes of reflex action by which such an irritation can produce paraplegia:—

1st. *Reflex Contraction of Bloodvessels.*—As it is now well established that bloodvessels contract with energy, and sometimes even are seized with a real and prolonged spasm, whether by a direct influence of their motor nerves, or through an excitation, which, from some centripetal or excito-motor nerve, has been reflected upon them by the cerebro-spinal axis, there is no need of showing here that blood-

vessels are just like muscles of animal life as regards their relations with the nervous system. This being the case, it is extremely easy to understand how a paralysis of the lower extremities, as well as that of any other part of the body, may be paralysed by a reflex action. In three different places a contraction of bloodvessels may cause paraplegia—1, in the spinal cord; 2, in the motor nerves; 3, in muscles. A contraction of bloodvessels in the spinal cord *I have seen* (in the vessels of the pia mater) taking place under my eyes, when a tightened ligature was applied on the hilus of the kidney, irritating the renal nerves, or when a similar operation was performed on the bloodvessels and nerves of the supra-renal capsules. Generally in those cases the contraction is much more evident on the side of the cord corresponding with the side of the irritated nerves, which fact is in harmony with another and not rare one, observed first by Comhaire (as regards the kidney), and often seen by me after the extirpation of one kidney or one supra-renal capsule—i. e., a paralysis of the corresponding lower limb. It is probable that irritations starting from the urinary and other organs produce a paraplegia by a contraction rather of the blood-vessels of the spinal cord, than of those of the motor nerves and muscles. However, in this form of paraplegia, it is not rare that a notable diminution of temperature of the paralysed limbs shows that the bloodvessels of these parts are also contracted.

2nd. *Morbid Reflex Influence on Nutrition.*—This influence, proved by many experiments on animals and by pathological facts of daily occurrence, seems usually not to exist in reflex paraplegia, except in that form in which muscles become progressively and rapidly atrophied and altered (*wasting palsy*). But this morbid influence may take place at any moment in the course of a reflex paraplegia, so long as the irritating cause has not ceased to act, and a myelitis or some other affection may be generated by it.

We think that it will now be considered possible, if not probable, that the production of reflex paraplegia is due to a contraction of bloodvessels and to the insufficiency of nutrition that follows this condition of the vessels. In addition to the reasons we have given to show that the paraplegia caused by an outside irritation is due to a reflex action and probably to a reflex contraction of bloodvessels, we will say that very often in this peculiar kind of paralysis there are reflex convulsions in muscles of the trunk, the head, and the limbs, before the paraplegia appears (as in cases of teething, of worms, &c.), or when it has already existed some time, (as on the passage of a catheter, in urinary paraplegia). We might also have pointed out the influence of the urethra on the spinal cord, by mentioning the well-known and very frequent fact of the production of chills and tremor on or after the introduction of a catheter in persons who are or who are not paraplegic.

There is another series of facts of which we have now to speak, that show that reflex paraplegia is not due to an evident organic

affection, which may be found in a post-mortem examination. We will very briefly mention some cases of autopsy made on persons who have died after having presented the symptoms of a reflex paraplegia.

Before concluding this lecture, I wish to say a few words on a most important point, to which we will again turn our attention in the succeeding lectures: I mean, the treatment of reflex paraplegia. I have already said that the various kinds of paraplegia may be classed into two general groups; one in which the amount of blood circulating in the spinal cord is too considerable; the other, in which the opposite condition exists. Of the various remedies, which, as I may perhaps be allowed to say, are too often blindly employed, there are some that diminish the quantity of blood in the spinal cord, such as mercury, ergot of rye, and belladonna; while others, on the contrary, such as strychnine and brucine, increase that quantity. If these last assertions be correct—and I will, by-and-by, try to prove that they are,—it is quite evident that to employ a remedy of either of these two kinds in both of the two groups of paraplegic cases is a most dangerous thing. Reflex paraplegia, as pointed out by its whole history given in this lecture, is accompanied and most likely produced by an insufficiency of the amount of blood in the spinal cord; it ought not, therefore, to be treated by those remedies which diminish the quantity of this fluid in the spinal nervous centre. How often, however, is not mercury employed in that affection? On the other hand, myelitis which is accompanied by an increase in the quantity of blood in the spinal cord, is often treated by the very remedy (strychnine) which has the most powerful influence in increasing the amount of blood in the spinal cord. How important, therefore, is it to know exactly to what group of affections (as to the quantity of blood in the spinal cord) belongs a paraplegia, before we give a remedy included in either of the two categories above-mentioned!

[Before speaking of the treatment of cases of reflex paraplegia, Dr. Sequard premises a few remarks on several forms of paraplegia presenting some analogies with reflex paraplegia.]

Amongst these forms or kinds of paraplegia, one of the most interesting is the paraplegia due to an obstacle in the circulation of blood in the aorta, or in its principal ramifications in the pelvis. This kind of paralysis has been well studied by veterinary surgeons, as it is frequent in horses. Several cases have been observed in man by Mr. Barth, Dr. W. W. Gull, and my friend Dr. Charcot. Besides the symptoms due to the cause preventing the circulation (generally an aneurism), this kind of paraplegia will be distinguished from the reflex paraplegia by the absence of the causes of this last form of paralysis, by the alterations of nutrition and pains in the paralysed limbs, and especially by the rapid increase of the paralysis after every notable exertion of the lower limbs, and the return of some power after rest.

There is another kind of paraplegia in which in some degree, a pressure on the large bloodvessels (arteries and veins) of the pelvis has a share in the diminution or loss of power. This kind of paraplegia exists frequently, in a slight degree, at the end of pregnancy; it is chiefly due to the pressure on the nerves of the lumbo-sacral plexus. During pregnancy, or in cases of a tumour in the pelvis, a complete paraplegia may be caused by pressure upon those nerves. This kind of paralysis of the lower limbs will easily be distinguished from the reflex paraplegia by the existence of violent pains in the pelvis and in many parts of the lower limbs, by the production of cramps, &c.

The so-called *idiopathic paraplegia* is not to be taken into account in this history of the differential diagnosis of the reflex and other forms of paraplegia. We do not know any case of paralysis of the lower limbs that deserves the name of idiopathic. Most of the pretended cases of that kind, published by Macario and by R. Leroy d'Etiolles, are either cases of reflex paraplegia or a real affection of the spinal cord (especially congestion and serous effusion). We do not mean to say that it will always be easy to decide during the life of a patient, or after a post-mortem examination, what is the cause of a paralysis of the lower limbs. We think that sometimes this cause will not be found out, and we only mean to state that it is impossible to admit the existence of a special kind of paralytic affection, deserving to be called *essential* or *idiopathic* paraplegia—an affection not more characterized by special symptoms than by special anatomical lesions.

Of other forms of paraplegia, such as those which are connected with gout, rheumatism, or those which follow grave fevers, cholera, &c., we will only say that usually they depend upon a serous effusion in the spinal canal or a venous congestion, and that sometimes they belong to the group of reflex paralysis.

In concluding our remarks on the diagnosis of the reflex paraplegia we need hardly say, that for this affection, as well as for all other diseases of the nervous system, it is not necessary to find together all the characteristic symptoms to be convinced of the existence of a reflex paraplegia. This affection is almost sufficiently characterized by *the absence of the special symptoms of an organic disease of the spine or its contents, and the existence of an incomplete paralysis of the lower limbs that has appeared somewhat slowly after a disease of the urinary or genital organs, or of some other abdominal viscus; after an inflammation of the lungs or pleuræ, or after some other kind of irritation of a nerve in its trunk or cutaneous ramifications.* In a great many cases of reflex paraplegia we shall find nothing else upon which to ground our diagnosis. But usually, in a short time, a much greater probability of the accuracy of the diagnosis will spring from the correspondence between changes in the degree of the paralysis with changes in the visceral disease or external irritation that is supposed to have produced the paraplegia.

The prognosis of the reflex paraplegia depends, in a great measure,

upon that of the external affection which has produced it. The whole history of the reflex paraplegia shows that it usually increases, diminishes, or disappears with the external morbid condition which has produced it. As a general rule, so long as this morbid condition lasts, the paraplegia persists, and it ceases very quickly, and sometimes without any special treatment, after the cure of this morbid condition. In some cases, however, paraplegia persists long after its external cause has been completely cured. It is so especially when the paralysis has been produced by cold, or by a disease of the bowels. The reverse is more particularly observed after the evacuation of worms, or after a cure of a stricture of the urethra, or of an affection of the womb.

Of the two forms of reflex paraplegia, one of which depends upon an insufficiency of nutrition in the spinal cord, the other upon an alteration of nutrition in the muscles of the lower limbs—the first is almost always curable when the external irritation which has caused it has ceased; while the second is very frequently incurable, whether its external cause has ceased or not. It is, therefore, most important for the prognosis to ascertain if there has been, or not, a rapid atrophy of muscles in cases of reflex paraplegia.

Treatment of the Reflex Paraplegia.—It is evident from all that we have said concerning the reflex paraplegia that, for this affection more than for any other, we must employ every effort to cure or alleviate the disease or irritation that has produced it. At the same time the paraplegia itself must not be neglected, and the proper means must be employed against it. Therefore the treatment of a reflex paraplegia ought to consist of two distinct parts: 1st, the means to be employed against the external cause of this affection; 2nd, the treatment of the paralysis itself. As regards the first part, we will say nothing here, as we cannot enter into the details of the treatment of nephritis, cystitis, pneumonia, enteritis, and other morbid states that may cause a reflex paraplegia. As regards the direct treatment of this affection, we will first lay down the general rules of the treatment, and then we will enter into the most important details concerning this treatment.

1. When it has been ascertained from what organ or from what nerve starts the nervous influence which causes a reflex paraplegia, besides the treatment that is appropriated to the nature of the local affection (of that organ or nerve), it is of the greatest importance to try to prevent or to diminish the transmission of any nervous influence from the diseased nerve or organ to the spinal cord. All the means usually employed to alleviate pain will be of service in such cases. If possible, we must try to paralyse for a time the sensitive nerves that convey the morbid influence to the spinal cord. Even a momentary suspension or diminution of the transmission of this influence may be very useful. Narcotics ought to be employed in

injections,—in the bladder, if that organ be the place from which starts that morbid influence; in the vagina, if the uterus be the place; and in the rectum, if the large intestine be the place. Narcotics ought to be taken by the mouth if the stomach, the small intestine, or the kidneys are affected. In case of a pneumonia producing a reflex paralysis, inhalations of chloroform (which, by the way, have been successfully employed against the inflammation itself) may prove useful. We will say by-and-by what narcotics should be preferred.

2. The object of the means just proposed is to diminish the cause of the paraplegia; the object of the means we will now speak of is just the same, although it may seem to be quite different. Excitants or revulsives applied to the skin of the legs have been warmly recommended by Graves, who has obtained good results from their use. Probably the mode of action of these means consists in producing for a short time the same effect as the irritation which is the cause of the paralysis—i. e., a contraction of the bloodvessels of the spinal cord: but, according to a well-established law, if such a contraction becomes considerable, the muscular fibres are soon exhausted, and a relaxation of the contracted fibres takes place, and, as a consequence of this relaxation, a dilatation of the bloodvessels occurs. Of all the causes of irritation capable of producing a contraction of bloodvessels by a reflex action, none has more power than cold. In consequence of this fact, I think some of the modes of application of cold to the spine ought to be employed in cases of reflex paraplegia. But the excitation in those cases must be very powerful, and able to produce a very considerable degree of contraction, so that the consequent exhaustion and dilatation may be obtained. With the same view, we may employ galvanism.

3. Another important principle, or rather another part of the same general principle of treatment, consists in making use of the following means to increase the quantity of blood in the spinal cord:—Every night, and often in the course of the day, the patient should lie down on his back, placing his head, his arms, and his legs on high pillows, so as to produce by gravitation a congestion in the spinal cord. This simple means, which is also applicable in cases of hysterical paraplegia, and in almost all the cases in which there is an insufficient amount of blood in the spinal cord, is just the reverse of what should be done in cases of inflammation or congestion of the spinal cord or its membranes, or of disease of the spine, &c., in which cases the patient ought to lie flat on the abdomen or on one side of the body, and have his feet and hands on a much lower level than that of the spine.

4. As regards the remedies to be taken by patients attacked with a reflex paraplegia, they must essentially be those which increase the amount of blood in the spinal cord, and augment the vital properties of this nervous centre, and also those remedies which render the blood richer in nutritive principles.

5. As regards food and the hygienic rules, patients attacked with a

reflex paraplegia must have the most substantial aliments, so as to improve the deficient nutrition of the spinal cord. They must take a great deal of exercise in the open air, and especially make use, as much as possible, of the paralysed muscles.

Before giving the details of the rules of treatment, we will sum up in the following propositions what we have just said :—

1st. Try to prevent, even for a short time, the continuation of the contraction of the bloodvessels of the spinal cord by the use of narcotics, to be applied, as much as possible, to the organs from which starts the external cause of the paralysis.

2nd. Try to produce by revulsives such a notable increase in the contraction of the bloodvessels of the spinal cord that an exhaustion of the contracted muscular fibres, and, as a consequence of this exhaustion, a dilatation of the bloodvessels, will soon take place.

3rd. Have the spine placed much lower than the head, the arms, and the legs, when the patient is lying in bed, so as to increase the amount of blood in the spinal cord.

4th. Employ those remedies that have the power of augmenting the vital properties of the spinal cord in increasing the amount of blood in this nervous centre.

5th. Give the kind of food and employ those hygienic means that are most fitted for increasing the quantity of blood and for improving nutrition in all parts of the body.

On the one hand, experience has proved all these rules of treatment to be the most effectual ; and, on the other hand, these rules are in perfect harmony with the theory of the disease that we have given : so that experience and theory furnish a confirmation to each other.

On Inflammation of the Spinal Cord.—To understand well the symptoms of inflammation of the spinal cord, it is important to bear in mind the following facts :—

1st. That the grey matter of the spinal cord in its normal condition is not at all *excitable*, and that irritations upon it are not followed by sensations or movements ; while, on the contrary, when inflamed it is excitable and able to give all kinds of sensations, and to produce cramps or partial convulsions.

2nd. That the white matter of the spinal cord is not composed of motor and sensitive fibres coming from or going to the brain.

3rd. That a pressure upon the spinal nerves, or upon the spinal cord, able to produce a paralysis accompanied by cramps, may either produce anæsthesia or not, while it causes various sensations.

It is not our intention to speak here of the acute myelitis accompanied by fever, as in almost all cases of that kind there is at the same time an acute inflammation of the spinal meninges, and also

because we only intend to show what the symptoms are in those cases of paralysis of the lower limbs of long standing that depend upon a chronic inflammation of a part of the spinal cord in the middle or lower parts of the dorsal regions.

The characteristic symptoms of this local myelitis are:—

1st. A constant pain at the part of the spine corresponding with the upper limit of the inflammation of the cord.

2nd. Whether a constant pain exists in a very marked degree or not, it is almost always found that pressure upon the spinous process of the vertebræ (sometimes even a slight one), when made at the upper limit of the inflammation, causes an acute pain.

3rd. The passage of a sponge, filled with warm water, along the spine, gives a normal sensation of heat in all the parts above the seat of the inflammation, but a burning sensation at its upper limit.

4th. The passage of a small lump of ice along the spine gives the natural sensation of cold everywhere, except at the level of the inflammation, where the sensation is that of burning.

5th. Most patients complain much of a sensation as if there were a cord, or some other ligature tied round the body, at the limit of the paralysis. In a few patients there is but a very slight sensation of that kind. This symptom seems to exist in all cases of myelitis, and to depend chiefly, but not entirely, upon a state of cramp of some part of the muscles of the abdomen or the chest.

6th. Various sensations, resembling very much those which follow the pressure upon a nerve, such as formication, pricking by pins and needles, and sometimes a feeling of burning or intense cold in the feet, legs, and thighs, less frequently in the abdominal walls. These sensations exist with as much, if not with greater, violence, in parts deprived of sensibility, as in parts which are still sensitive. They originate from the irritation of the grey matter of the spinal cord, and are *referred* to the limbs and abdomen, just as the pressure upon a nerve produces sensations in its ramifications. They are important indications of myelitis.

7th. Cramps in the feet or calf of the legs are very frequent. There are more or less of them in every patient. Frequently there are cramps also in the large abdominal muscles, besides the circular and linear cramp that gives the above-mentioned feeling of tightening. A cramp limited to a part of one or several abdominal muscles, may remain almost permanently for days and weeks, forming a kind of lump, which may be mistaken for a tumour.

8th. Whether myelitis exists only in a small zone of the spinal cord, or occupies the whole of the dorso-lumbar enlargement, the paralysis of movement exists in all the parts of the body that receive their nerves from the portions of the spinal cord that are below the upper level of the inflammation. The degree of paralysis varies extremely in different patients, but it is nearly the same in the various muscles of the lower limbs in the same patient.

9th. Paralysis of the bladder and of the sphincter ani is almost always present in inflammation of the lower part of the dorsal region of the spinal cord; but when the seat of the inflammation is higher up in the dorsal region, there is rather a spasm than a paralysis in the sphincters of the bladder and anus. Often then there is retention of urine, owing to the paralysis of the bladder while the sphincter vesicæ is more or less in a spasmodic state.

10th. One of the most decisive symptoms of myelitis is the alkalinity of the urine. There is no patient attacked with myelitis in the dorsal region of the cord whose urine is not frequently alkaline. At times, especially after certain kinds of food, the urine is acid, but the alkalinity soon reappears.

11th. Anæsthesia, or at least a diminution of sensibility, always exists in myelitis, except when the grey matter is not the seat of the disease, which is very rare. Usually, the inflammation begins in the central parts of the grey matter, and then a diminution of sensibility is one of the first symptoms. That peculiar kind of sensibility of muscles which serves to direct our movements is especially impaired in the very beginning.

12th. When the dorso-lumbar enlargement is inflamed, reflex movements can hardly be excited in the lower limbs, and frequently it is impossible to excite any. On the contrary, energetic reflex movements can always be excited when the disease is in the middle of the dorsal region, or higher up.

On the Treatment of Chronic Myelitis.—[Myelitis, in the acute form is almost always fatal. But the subacute and chronic forms are frequently stopped in their progress, when properly treated, and sometimes an almost complete cure may be obtained. Any actual injury, however, which the cord may have sustained, is almost irreparable. Chronic myelitis is an affection of very slow development, and still slower progress. In the treatment, the first and most important rule is, to diminish the congestion of the spinal cord. To this end the patient must avoid those positions of the body in which by gravitation the amount of blood in the cord will be increased. Hence the patient must never lie on the back. Dry cupping daily on the various parts of the spine will prove of service.]

Amongst the remedies to be employed internally, the most active are those which have the power of diminishing the congestion of the spinal cord. The two which seem to be most powerful in this respect are belladonna and ergot of rye. Experiments upon animals have shown to me, in the most positive manner, that these two remedies are powerful excitants of unstriped muscular fibres, in bloodvessels, in the uterus, in the bowels, in the iris, &c. Both of them dilate the pupil; both are employed with success to produce contractions of the uterus; but each of them has more power in certain parts than the other, so that we find belladonna acting more than ergot on the blood-

vessels of the iris (which is the principal cause of the dilatation of the pupil)—on the bloodvessels of the breast (which is the principal cause of the cessation of the secretion of milk)—on the muscular fibres of the bowels (which is the mode of its action in cases of strangled hernia)—on the sphincter of the bladder (which is one of the causes of its success against nocturnal incontinence of urine), &c.; while, on the contrary, we find that ergot acts more than belladonna on the muscular fibres of the womb, on the bloodvessels of the spinal cord, &c. We cannot give here the proof of the exactitude of these assertions on the mode of action of these two remedies, but we must at least answer an objection which probably will arise in the mind of many persons. It will be asked—How is it that, of two remedies that are able to excite contractions in smooth muscular fibres, one produces them more in one place, and the other more in another place? The answer to this objection is indeed very simple. The excitability of smooth fibres, as well as that of striated muscles, varies exceedingly in different parts of the body. An exciting agent (whether galvanism, cold, heat, or belladonna and ergot of rye) will produce powerful contraction in some places, and hardly any in other places. The smooth fibres of the uterus contract more than those of the bowels or the bladder, and less than those of certain bloodvessels when stimulated by galvanism; the smooth fibres of certain bloodvessels contract more than those of the uterus under the excitation of cold; still more, the bloodvessels of the cerebral lobes and of the face, which contract so much when their nerve (the cervical sympathetic) is irritated, contract but very little when excited by belladonna and ergot, while these two excitants produce powerful contractions in the bloodvessels of the spinal cord.

Not only have I seen the diminution in the calibre of bloodvessels of the pia mater of the spinal cord taking place in dogs after they had taken large doses of belladonna or ergot of rye, but I have also ascertained that the reflex power of the spinal cord (most likely as a consequence of the contraction of bloodvessels) becomes very much diminished under the influence of these two remedies, which in so doing act just in the opposite way to that of strychnine.

Led by the knowledge of the above facts, we have employed belladonna and ergot of rye in cases of paraplegia due to a simple congestion or a chronic inflammation of the spinal cord and its meninges, and we have obtained a greater success than we had dared to hope for. Whatever be the value of our experiments on animals as regards the mode of action of these remedies, it is now certain that they have really a great power in diminishing the amount of blood in the spinal cord and its membranes. It is very well known that many French physicians, especially Bretonneau, Payan, Barbier, and Trousseau, have for many years employed with success belladonna and ergot of rye in cases of paraplegia. Of course we do not claim to have been the first to make use of these remedies in paraplegia, any more than

we claim priority as regards employing strychnine in various forms of paralysis. But we claim to have pointed out, as clearly as we could, in what cases of paraplegia strychnine or belladonna and ergot of rye are to be employed or avoided. To indicate this distinction is the principal object of these lectures.

In the beginning of the treatment of chronic myelitis, we usually employ ergot of rye alone internally, and belladonna externally in a plaster applied to the spine, over the painful spot. The dose of ergot, when the powder is used, which is almost always the case, is at first two or three grains twice a day; gradually the dose is increased until it reaches five or six grains twice a day; and in a few cases we have given eight grains twice a day. We do not think it is necessary to make use of the very large doses employed by M. Payan. The belladonna plaster applied to the spine must be a very large one, four inches wide, and six or seven inches long. If there is no amelioration in a few weeks, we give the extract of belladonna internally in doses of a quarter of a grain twice a day.

When we find that the patients, after six or eight weeks of treatment by ergot of rye and belladonna, do not get better, we give iodide of potassium in doses of five or six grains twice a day, in addition to the preceding remedies. When there is any reason to suspect that there is a degree of meningitis together with myelitis, we begin at once the treatment by the iodide of potassium with the ergot and belladonna.

On Paralysis of the Lower Extremities.—[One of the most frequent causes of this kind of paralysis is probably inflammation of the spinal cord; another cause is softening, caused by alterations in the bloodvessels preventing a proper interchange of matter between the nervous tissue and the blood.]

Usually patients attacked with the non-inflammatory softening of the spinal cord at first complain only of weakness. They gradually become weaker, although their general health may seem to be as good as ever. They feel the weakness especially at the knee and ankle-joints; they find it very difficult to go up or down stairs, to go in or out of a carriage, and their gait becomes tottering, especially when they cannot direct their movements with the help of the sight. When lying down, if they see the lower limbs, they can move them pretty freely, but without force. After a time the paralysis becomes much more marked, and then sensibility in the whole length of the lower limbs and the power of the will over the bladder and the rectum are also found much diminished. There is no pain, or hardly any, in the spine or paralysed parts. Very rarely is the urine altered. The temperature of the lower limbs is often higher than it is in health, especially when the paralysis is complete or nearly so.

Hemorrhage in the Grey Matter of the Spinal Cord.—Paraplegia due to this cause is characterized by the suddenness of its production,

by a pain at the part of the spinal cord where the hemorrhage occurs, owing, probably, to the distension of the posterior roots of the nerves in the grey cornua, by a pain in the spine after pressure upon it, and also by a pain in the parts of the body where are distributed the nerves originating from the part of the spinal cord which is the seat of the disease. The sphincters of the bladder and rectum are almost completely paralysed from the very first. It is not rare that an inflammation supervenes in the parts of the cords surrounding the clot, and then all the symptoms of myelitis occur.

According to the seat and the extent of the hemorrhage the symptoms vary extremely. If the amount of blood effused be very small, the symptoms may be very slight, and the patient may recover in a few months, as in a case recorded by Cruveilhier. If the amount of blood is considerable, it descends along the whole length of the central canal of the spinal cord, and also sometimes breaks through the grey cornua, especially the anterior ones. The loss of sensibility and of voluntary power is then complete, and the temperature of the paralysed limbs is increased. If the hemorrhage is limited to one lateral half of the grey matter,—as in a most interesting case recorded by Monod, and in another by Mr. Oré,—there is loss of movement in the side of the hemorrhage, and loss of sensibility in the opposite side.

It is a remarkable feature of paraplegia due to hemorrhage in the grey matter of the spinal cord that a diminution of sensibility is always one of its first symptoms; and that when the whole of the grey matter is altered by the effused blood, the anæsthesia is complete.

Diagnosis of the Hemorrhage in the Spinal Cord.—The suddenness of the production of paraplegia is almost sufficient to show that it is due to effused blood, but it remains to decide whether the blood is in the tissue of the spinal cord, or in the vertebral canal, outside of this nervous centre. In this last case, there is more pain in a great extent of the spine than in the preceding; and also tetanic convulsions are not rare when the blood is effused outside of the spinal cord; while a hemorrhage in the cord has never produced convulsions, so far as I know. Besides, there is more marked anæsthesia in cases of effusion of blood in the tissue of the spinal cord than outside of it.

It is usually very easy to distinguish a hemorrhage in the spinal cord from a white softening of this organ. The suddenness of the production of the paralysis and the existence of the pains will sufficiently characterize the first of these affections; and in those cases of almost sudden paraplegia, owing to softening, the absence of pains (in the spine, &c.) will serve to distinguish this form of paraplegia from that due to a hemorrhage. I need not say that these two affections—i. e., softening and hemorrhage—do not rarely co-exist. This co-existence will not be surprising to any one who knows that in the spinal cord, as well as in the brain, the same morbid condition of bloodvessels is usually the cause of both these affections.

Prognosis of Paraplegia due to White Softening or Hemorrhage in the Spinal Cord.—The great indication for the prognosis in cases of softening or hemorrhage in the spinal cord is the extent and degree of the paralysis. A softening limited to the lower part of the dorsal region of the spinal cord may admit of a very prolonged duration of life. A hemorrhage is a more grave affection than a softening, because there is always a great chance of its being reproduced, and also because the blood effused may cause very extensive alterations. We do not know a single case of complete cure of paralysis of the lower limbs caused by softening or hemorrhage in the spinal cord; but, as far as diagnosis not confirmed by autopsy may be relied on, we can say that we have seen many cases of non-inflammatory softening of the spinal cord arrested in their development, and still more, that in five or six cases, a very notable amelioration has taken place.

Treatment of Paraplegia due to White Softening of the Spinal Cord.—The treatment of this affection ought to be pretty much the same as that of hemiplegia caused by a non-inflammatory softening of the brain. We will not enter here into the discussion of the various modes of treatment which have been proposed or employed in this last case; we will only mention the general plan of treatment we have employed with some appearance of success in many cases of softening of the spinal cord.

1st. Iodide of potassium is the principal of the remedies that may be relied upon. We generally employ it in doses of five grains twice a day, mixed with nearly the same doses of sesqui-carbonate of ammonia, in a decoction of cinchona bark, or an infusion of calumba or rhubarb. We insist upon the importance of taking this mixture before food in the morning and an hour before dinner, to avoid the decomposition of the iodide by the gastric juice and the setting free of the iodine, which sometimes takes place, and causes a gastric disturbance, erroneously attributed by some practitioners to the iodide itself.

2nd. Besides the preceding remedy, we prescribe various tonics, such as iron and cinchona-bark wine.

3rd. Strychnine may be employed with some profit in those cases where the paralysis is slight; but it must be remembered that, on account of the congestion produced by large doses of this alkaloid in the spinal cord, there would be danger in making use of it in cases where, the paralysis being complete, or nearly so, there is reason to think that the bloodvessels of the spinal cord are much altered, and that therefore a hemorrhage might be the consequence of the action of strychnine. I need hardly say that those remedies which diminish the amount of blood in the spinal cord (such as belladonna, ergot of rye, &c,) should not be employed in this form of paraplegia.

4th. Besides the use of some of the above remedies, I recommend, as an excellent means of improving the nutrition of the spinal cord, the daily use of the cold douche or the cold shower bath applied to he spine. Sea-bathing may also be of service,

5th. The patient should lie down flat on his back at night; he should live upon the most nutritious food, drink wine and beer in a moderate quantity, and take as much exercise in the open air as possible, without however exhausting his diminished power of motion. Shampooing and galvanism may be applied with profit to the paralysed limbs.

Treatment of Paraplegia due to Hemorrhage in the Spinal Cord.—The same rules are to be followed in cases of this kind of paralysis as in cases of softening of the spinal cord, with these slight differences: that, 1st, three doses of iodide of potassium, instead of two (of five grains each), ought to be given every day; 2nd, strychnine ought not to be employed; 3rd, constipation, lying down on the back, and all other causes of congestion of the spinal cord, should be carefully avoided.

Symptoms and Treatment of Paraplegia due to Hemorrhage in the Vertebral Canal.—A hemorrhage between the spinal cord and its bony envelope is fortunately an event of rare occurrence. I have seen but one case of this kind of hemorrhage, which proved fatal in less than two days. The blood is effused either between the pia mater and the arachnoid, or (more rarely) between the dura mater and the vertebræ. The first symptom is usually a pain extending along the spine from the seat of the hemorrhage to the lower extremity of the vertebral canal. Almost immediately a complete paralysis of the lower limbs and very often tetanic convulsions appear. In almost all cases this hemorrhage is rapidly fatal, either in consequence of its influence upon the circulation or respiration, or, indirectly, in consequence of an acute and extensive meningitis. Our object being here to study paraplegia in its chronic forms, we will not say more upon the symptoms of this affection.

As regards the treatment of the hemorrhage in the vertebral canal, we will only say: 1st, that, at once, all the most active means usually employed in the various cases of visceral hemorrhage should be made use of; 2nd, that the patient should be placed in bed on one side, and not on his back; 3rd, that pounded ice should be applied, in bladders, all along the spine; 4th, that if the patient survives several days, the same treatment as is above prescribed for cases of hemorrhage in the grey matter should be employed.—*Lancet, April* 28, *May* 5, *June* 9, *July* 14, *Aug.* 18, *and Sep.* 8, 1860, *pp.* 415, 437, 563, 27, 155, 229.

8.—ON TETANUS.

By Frederick C. Skey, Esq., F.R.S., Surgeon to St. Bartholomew's Hospital.

[All the known facts of tetanus go to prove that the disease is centered in the nervous system, and that the circulatory system is but secondarily affected.]

No sooner is the nervous centre sent to sleep than the disease appears as suddenly to cease. Notwithstanding this improvement, so deceptive and insidious, it is obvious that morbid actions predominate, and I doubt whether such temporary improvement, plausible and indeed marvellous as it is, can be said to indicate an arrest of the disease in however slight a degree.

A boy, aged fourteen, was brought into St. Bartholomew's Hospital, with a hand greatly mutilated by machinery. The house-surgeon decided, on consideration,—and I thought wisely decided,—to make the attempt to restore the hand. For twelve days he progressed favourably. On the thirteenth, he complained of stiffness in the back of his neck. When I saw him, at the expiration of some hours, spasm of the muscles had extended to the chest and abdomen; but none of the muscles were severely or painfully contracted. Still the signs were distinct. The boy was the subject of tetanus; and as I had never had a successful case, I was the more anxious to adopt some more promising agent than any I had hitherto employed. With this view I determined to test the efficacy of the woorara poison, which had been employed with some effect by Mr. S. Wells in three cases of tetanus. By the kindness of Mr. Savory, who had a small quantity of the fresh poison, I was enabled to obtain sufficient for the purpose. We dissolved two grains in an ounce of distilled water. The boy being placed under the influence of chloroform, I amputated the hand immediately above the wrist-joint. My intention was to have applied two drops of the solution to the wound, and, observing the consequences, to have increased the quantity applied according to the effect produced. On recovering his consciousness, it was obvious that what had been done for the boy, whether the removal of the hand or the administration of chloroform, had been in the direction of good, the pulse had fallen from 130 to 100, and the rigidity of the muscles was greatly diminished, and continued so. The pain was also reduced, and the countenance was more tranquil. He could open his mouth to an extent sufficient for the introduction of food. The rigidity of the muscles was by no means great, nor did he suffer the degree of pain usually attendant on severe cases of tetanus. It is very true that the sterno-mastoids, pectorals, abdominal muscles, and, indeed, the muscles of his trunk generally, were involved; but their contraction was comparatively slight, and certainly was unproductive of considerable pain. The use of the woorara was postponed, and I ordered the boy tincture of opium, ten minims; compound spirit of ammonia, half a drachm; brandy, half an ounce, every two hours. On the following day I found he had had a tolerably good night; his pulse had slightly risen in frequency; the rigidity of the muscles had not increased; on the contrary, it appeared somewhat lessened. His countenance was more anxious, which appeared rather attributable to mental than bodily suffering, for he had only recently become cognizant of the removal of his hand. By two o'clock on that day, the boy had con-

sumed four ounces of brandy, half an ounce of spirit of ammonia, and eighty drops of laudanum. Still I had no excuse for the trial of the woorara. Doubtless the boy was in a dangerous condition; but it was obvious that the condition was not attributable to the morbid contraction of the muscular system, but to something beyond it. His bowels were constipated. I ordered him calomel and jalap, castor oil, enemata of turpentine, croton oil in no insignificant doses; but I failed to obtain the desired result up to the time of his death. In other respects, the same treatment was continued. On the third and fourth days his condition was not materially changed. He took food without difficulty, as also his medicine at the required intervals. The ammonia was increased to drachm doses. On the fourth day, his pulse had increased to 135, and his countenance was more pallid. The rigidity of his muscles had not increased in any positive degree, and those of the extremities were entirely free from spasm. On the morning of the fifth day he had taken, without producing any marked effect on his system for good or for evil, about forty-eight ounces of brandy, one ounce of tincture of opium, and four ounces of spirit of ammonia. And we may form some judgment of the severity as well as the prostrating influence of the disease under which this poor boy was labouring when we consider that such an enormous power was inadequate to produce the smallest impression on his system. On the last visit I paid, except that he appeared somewhat weaker, I cannot say that he was positively worse. He could open his mouth sufficiently wide for the introduction of nourishment, and he took food freely throughout the greater part of his illness. His pulse was a shade weaker, but not materially so. His spasms were not severe nor considerable, and assuredly they were not a source of pain. Of this the boy gave me full assurance on many occasions. On the evening of the fifth day he died. His body was not examined.

If the nature of the disease we call tetanus could be ascertained by dissection, we should have acquired this knowledge long ago; but, so far as I know, we have obtained no information by such means, except negatively, on which to found a theory, or to obtain a guide to practice. The total amount of knowledge consists in this: that at varying intervals after the occurrence of more or less violent local injury, and occasionally—but in our latitude very rarely—independently of injury, we observe rigidity of the muscular system, commencing with the muscles of the jaw and extending to the neck, trunk, and finally to the extremities; that it is accompanied by great pain in the contracted muscles, a quick pulse and respiration, and constipated bowels; and that it terminates, in from two to six or eight days, in death. Such is nearly the sum total of our knowledge of tetanus, and it may be doubted whether we are even now occupied in the right path of inquiry. We fix our attention on the local spasm of the muscular frame, as though that condition of the system constituted the essence of the disease, of which it is only a symptom. We rather

search for remedies that will abate spasm, than for such as will attack its cause. The above case exemplifies negatively this fact. The boy died of tetanus, but not of spasm. At no period of his case was the contraction of his muscles so rigid as to cause pain of a severe character. He complained of his arm; he suffered from a sense of illness; he desired to be let alone, and not to be disturbed; his expression betokened serious illness; but he did not die from pain nor from spasm: he gradually sank from utter prostration of nervous power, which every remedy employed was incompetent to contend against. And it is by no means uncommon to observe, towards the close of life, that the most prominent symptom of the disease—namely, muscular rigidity—gradually subsides, while the real disease advances towards its fatal crisis. In the whole range of disease, I do not recollect any evidence of the existence of what we term the nervous system more remarkable or more conclusive than is derived from the influence of anæsthetics in tetanus. How absolutely is the morbid influence of the nervous system on the heart and respiratory system suspended by chloroform! and how suddenly too! The pulse falls to its natural standard, the equilibrium of respiration is restored, all expression of suffering subsides under the reality of tranquil sleep. So complete is the external manifestation of health, that it would astonish a bystander, ignorant of the nature of the case, to be assured that in a few days, possibly a few hours, it would terminate in death. Suspend the remedy, remove the influence of the anæsthetic agent, and the entire train of fatal symptoms starts, as it were, into existence—the pulse rises, the respiration is quickened in proportion, and the muscular spasm, with its usually inseparable companion, pain, become again dominant. Nor do we observe that the disease, to whatever extent it may have reached, is retarded or even diminished by the treatment. On the contrary, its progress is unremitting; and both the violence of the spasm and the severity of the pain are regenerated with an increase of intensity proportioned to the duration of time during which they have been suspended by anæsthetic agency. In the above case, I was fully prepared to test the curative properties of the woorara poison, as recorded by Mr. Wells and recent French writers; but I really had no excuse for its employment. The boy did not die either from or with spasm; and I cannot persuade myself that we shall ever discover in the woorara poison a curative property capable of rousing the vital powers, already oppressed by mysterious morbid actions going on within the frame. Still, I am quite prepared to acknowledge that all reasoning, more especially on a subject so obscure as tetanus, sinks in comparison with practical observation. Other authorities may view the subject in a different light; and it will prove no small accession to our knowledge should we discover in this or in any other untried agent the power to arrest so hitherto fatal a disease as that which forms the subject of the above brief remarks.—*Lancet, July* 7, 1860, *p.* 1.

9.—ON THE TREATMENT OF ACUTE TRAUMATIC TETANUS BY TINCTURE OF ACONITE.

Of all the vegetable preparations, aconite is known to be one of the most powerful in its action upon the spinal nervous system; hence its value in tetanus ought not to be inferior to the many other anodyne remedies which are occasionally employed. In the pages of this journal (vol. i., 1846, p. 484) we have recorded an instance, under the care of Mr. Page, at the Cumberland Infirmary, Carlisle, in which a recovery took place under the use of Fleming's tincture of acoonite. Another example also appears (ibid, p. 494) from Mr. Newton, of Newcastle, treated by the same remedy, which proved unavailing, for death ensued. We now give another, in which life was saved by the administration of aconite; but whilst we feel disposed to give due credit to the influence of the drug upon the disease, we can hardly attribute the entire cure to it, when the period at which its employment was commenced is remembered, for the tetanic symptoms had been existing twelve days before it was had recourse to.

We recollect a case of acute traumatic tetanus at Guy's Hospital in the summer of 1858, under the care of Mr. Cock and Dr. Wilks, treated by as many as 110 doses of cannabis indica; but the cure was attributed more to the quantity of wine and beef-tea taken than to the medicine. In Mr. Fergusson's second case, reported to-day, aconite was found ineffectual, and woorara was substituted for it, with, however, no better result.

The patient in the present case, a lad aged 15 years, had always enjoyed good health. On the 30th of August, 1858, while wearing thin boots, he trod on a rusty nail, which pierced the ball of his foot. He withdrew it directly; the wound bled but little, and healed in a few days. On Sept. 6th the boy felt stiffness about the neck and lower jaw, which increased daily, and on the 12th he was unable to open his mouth, and could not walk owing to the stiffness of his back. He was admitted into the Hospital on the 16th. There was then well-marked rigidity of the muscles of the neck and jaws, and the risus sardonicus was characteristic. He was unable to turn his head or separate his jaws; abdominal muscles very tense; complains of much pain in the neck and back; has not slept for two or three nights; bowels open; perspiring; pulse 80, moderately full. A hard cicatrix in the sole of the foot was excised. He was at first put upon strychnine, one-tenth of a grain, every two hours; carefully watched; soon diminished to one-twentieth of a grain, but the symptoms continued to increase. Twitchings began in the thighs, and great difficulty of respiration, and finally severe general spasms, with well-marked opisthotonos.

On the 20th, the plan of treatment was changed, and tincture of aconite was given, at first five minims every two hours, then eight minims. This was continued till the 27th. The dose was then

administered every four hours, and on the 28th every six hours, and on the 6th of October it was left off altogether. The diet was throughout the most nourishing that could be taken—strong beef-tea, brandy, &c., and the bowels were kept open by turpentine injections. From the time the aconite began to take effect an improvement took place in the severity of the symptoms, and he has been steadily and gradually progressing, though very slowly. First the general spasms and opisthotonos ceased, then the convulsive twitchings of the extremities; these lasted till about the 1st of October, at which date he could sit up for his meals, could separate his teeth about half an inch; ate and slept well: face almost regained its natural expression.

Has been slowly convalescing since then. Can now (Oct. 22nd) walk about, and appears in good health and spirits; only still feels a little stiffness about the muscles of the back, and cannot yet quite separate the jaws as wide as natural.

He was discharged from the hospital well on Nov. 2nd.—*Lancet, Aug.* 18, 1860, *p.* 162.

10.—ON THE TREATMENT OF HEMICRANIA BY INJECTION OF MORPHIA.

By JOHN K. BARTON, M.B., Surgeon to the Adelaide Hospital.

[The following case is interesting, as showing the successful results which in many cases follow the employment of the hypodermic administration of morphia. The patient was a pale anæmic-looking woman, debilitated from menorrhagia. A first attack of hemicrania was cured by quinine, but it returned worse than before, and the same treatment was again tried, and failed. Chloroform and opium relieved the pain, but only for a time.]

Upon the 2nd of March I dissolved a grain of the muriate morphia in a drachm of water, and injected exactly ten drops of this into the subcutaneous cellular tissue of the back of the neck, selecting this place as the patient pointed it out as that from which the pain seemed to proceed. The operation was performed without giving scarcely any pain to the patient, and with perfect accuracy as regards the exact quantity injected, by means of the graduated glass syringe, with a sharp-pointed perforated nozzle, as originally I believe used by Dr. Wood. In about an hour after this injection the pain had disappeared; it returned slightly towards evening, but she passed a good night, sleeping more soundly than she had done since the attack commenced.

Upon the 3rd the injection was repeated, the chloroform draughts being entirely omitted. The note taken upon the next day stated that the pain had entirely disappeared after the second injection, and she had remained free from it until evening, when there was a return of it for about an hour, but that she slept soundly and awoke free from pain—a feeling, as if the skin was sore along the side of the neck and

head, remained for a day or two. Upon the 9th there was a return of the pain, whereupon ten drops of the same solution was again injected, with the effect of giving immediate relief, the pain returning at night for about half an hour, as upon the 5th, and disappearing entirely again.

Upon the 12th the patient at her own request left the hospital, being quite free from pain since the last injection, and I have not heard of her since.

The great severity of the pain in this case caused the effect of each remedy tried to be more distinctly seen than in other less marked cases, and no doubt the morphine injection was the first of the many means employed which gave decided, and more important still, permanent relief, for the pain did not return with the same intensity after this treatment was employed, and after the third injection it disappeared altogether. I confess that this was more than I anticipated, for I had tried the same treatment in two or three cases before this, and in most of them I had obtained temporary relief only; but some consideration of the nature of these cases may perhaps explain this. *First*, I employed it in a case of severe and long-continued sciatica in a strumous unhealthy countryman, who had been constantly exposed to wet and cold in cutting turf, and other country labour. Repeated blistering, &c., and cupping had been put in operation before he had come to town, without any effect in removing the pain. The injection was repeated several times, and was always followed by an abatement of the pain, which sometimes lasted for several days. I found it necessary, however, to resort to some other treatment, and gave him the corrosive sublimate in decoction of bark, and under this, with the continued injections, he recovered so far as to leave the hospital and return to the country nearly quite free from pain.

Secondly, I employed it in another case of sciatica occurring in a middle-aged healthy woman, who had got a very severe wetting. She had sat in her wet clothes, and shivered before the attack of pain came on. In this case relief was obtained from the injection for two or three hours after it was employed, but no permanent benefit arose from it. Again, I employed it in a case where there was intense pain in the lumbar region, arising from caries of the lumbar vertebræ, hoping to relieve the pain until the issues which had been inserted should begin to have their effect; but without the least benefit, even temporary, resulting.

From these cases it would appear that this treatment was of no use whatever in a case where there was active inflammatory disease (caries of bone) producing the pain; that it was of temporary benefit in a case of severe pain, originating in inflammation of the nerve itself; that it was of more decided benefit in a case of the same kind, but in which the inflammatory symptoms were less marked; and finally, that it was of very marked and decided benefit in a case where

there was no inflammation whatever, and where the pain was of a purely neuralgic character, depending upon general anæmia.

In such cases as this latter I would therefore recommend its employment very decidedly, ten drops of a solution, such as I employed, with one grain of the muriate or acetate of morphia to a drachm of water, will be found quite enough. Women are more easily affected than men, so that ten drops is as much as ought, under any circumstances, to be injected when the patient is a female. Care should be taken that the needle has penetrated into the cellular tissue beneath the skin before the injection is made, otherwise the solution will flow out when the instrument is withdrawn.—*Dub. Hospital Gazette, Aug.* 1, 1860, *p.* 229.

11.—ON THE HYPODERMIC ACTION OF MORPHIA.

By JOHN K. SPENDER, Esq., Surgeon to the Eastern Dispensary, Bath.

[The following is an outline of four cases of neuralgia, in which the author administered a solution of the acetate of morphia by the hypodermic method.]

Case 1.—A female, aged about 35, of spare and delicate habit, came first under my care in 1857, for acute rheumatism; subsequently, for hemicrania and other quasi-rheumatic pains; and lately, for sharp and persistent pain in the inframaxillary nerve of the left side. I need not enumerate the various devices, local and constitutional, which were resorted to; suffice it to say, that they gave scarcely any relief, even for a few hours. At length, I determined to inject some morphia under the skin, and first gave, according to this plan, ten drops of the Pharmacopœial solution of acetate of morphia. This caused sound sleep for two or three hours, which was followed by considerable remission of suffering. On the next occasion, I injected fifteen drops of the Pharmacopœial solution, and afterwards increased it, dose by dose, to twenty-two drops, which was the quantity introduced last Friday evening. The ultimate effect now resulting is complete relief from pain for about three days, and it then returns with less intensity than before. There are particular circumstances about this case which render it unlikely that the neuralgic disorder will ever be cured; but it is noticeable that whenever morphia was given by the mouth, no appreciable effect on the pain was ever produced.

Case 2.—The second case was that of a lady, aged about 50, who had for many years past suffered from paroxysmal attacks of neuralgia in the upper maxillary nerve, especially in the spring and autumn. All that I had been able to do, when consulted by this patient on several occasions, was to bring about a gradual removal of the pain by iron and quinine, and thus to alter the condition of blood on which the disease might be supposed to depend. She was always an anæmic

subject; and, furthermore, often had her sleep disturbed by being obliged to wait on a sick husband. About a month ago, I was sent for to see her one evening during an intense neuralgic attack. I took my apparatus with me, and injected at once fifteen drops of the officinal solution of the acetate of morphia. Next day, I found that the operation had been followed by sleep for six hours, with almost complete relief from pain; the only complaint now made being some amount of soreness over the course of the affected nerve. Complete recovery took place; and it is to be remarked that in this case the administration of narcotics in the ordinary way had only stupified the sensorium, without in any way reducing the local hyperæsthesia.

Case 3.—My third case occurred in Dispensary practice, and was a poor middle-aged woman, who is suffering from apparently malignant disease of the orbit. The eye-ball is pushed forward, the left side of the frontal bone is raised, and the malar bone of the same side is protruded downwards and outwards. The pain is often of an excruciating kind, and had scarcely at all been alleviated by enormous doses of opium. It was obvious that the utmost that therapeutics of any sort could do must be of a bare palliative kind; but I injected one day fifteen drops of solution of acetate of morphia, with considerable relief, though at the expense of such severe sickness, that I was not allowed to repeat the process. The pain, of course, soon returned, and there does not seem anything further to be done.

Case 4.—A few days ago I injected ten drops of the solution of acetate of morphia into the areolar tissue of an elderly gentleman who has long suffered from subacute neuralgic disorder, attributable to suppressed gout. The result was an agreeable determination to the skin, and a generally soothed state of the nervous system.

The region which I have always selected for the injection of the solution is the areolar tissue of the upper arm; but the posterior part of the neck would probably answer equally well. Mr. Hunter has described the injecting instrument, and the mode of using it. It consists of a glass barrel, with a pipe made of silver, which screws on and off the barrel at pleasure: the end of the pipe being sharpened like a needle, and perforated on one side by an oblique opening, through which the drops of the solution are expelled. Having charged the syringe with the narcotic fluid, hold it in the right hand at the junction of the barrel with the pipe, and with the left hand take up, between the finger and thumb, a fold of the skin of the patient: having made it tense, let the point of the syringe be passed through it with a quick steady movement, and sufficiently deep that the point can be distinctly felt under the finger. The piston must now be pressed very slowly down with the thumb of the right hand, and the escape of the fluid into the areolar tissue will be indicated by a circumscribed elevation of the epidermis. After the instrument is

withdrawn, a small strip of plaister should be placed over the aperture caused by the needle.

According to the best authorities, we should never use more than half the ordinary stomachic dose for males, nor more than one-third for females. The specific action of any medicine is developed with much greater certainty when it is introduced hypodermically, chiefly because the whole quantity of a substance given in this way is absorbed, without being submitted to the dynamic influences of the gastric and intestinal secretions.

I regret to say that *intense sickness* is a frequent and troublesome sequel of the hypodermic injection of morphia. In three of the cases above related, this symptom was very severe, and in some measure obliterated the sense of relief otherwise obtained. In one instance, a temporary cutaneous eruption, like urticaria, was also noticed.

A due discrimination of cases ought to be made for this plan of treatment. To practise it in every example of neuralgia would be unwise and useless; for nerve-disorder often depends upon such trivial causes as an error of digestion or over-fatigue, and vanishes spontaneously when the exciting agent is removed. The cases in which success can most probably be hoped for are those in which the system has been long worn by pain, and in which a general spanæmic condition exists. The administration of morphia in the hypodermic method appears then to act as a narcotic shock, which powerfully and almost instantaneously soothes the whole nervous system, and destroys the local hyperæsthesia. Sometimes this result is permanent, sometimes not, according to the permanent or temporary agency of the causes of the disorder. But however short the interval of ease, it ought to be very welcome; for I believe that the great hindrance to our successful treatment of neuralgia has been our obstinate theorising about its etiology. While we are busy in classifying our supposed *materies morbi* (no classical phrase has ever exercised so complete a tyranny over us), and are applying our chemical and dynamical drugs accordingly, we are apt to forget the simple clinical fact that a human being lies in agony by our side, and supplicates relief for that one symptom —*pain*. Let us, if we can, first utterly overwhelm and abolish this pain; perhaps it will never return; but at all events, when our patient is in a grateful stupor, there will be time to philosophise about the origin of the malady which we are called upon to subdue.

It must be admitted that there are certain trifling practical difficulties in the way of the hypodermic administration of medicines. The introduction of a pointed instrument under the flesh causes a momentary pang, which cannot be evaded. We may liken this pang (as we can with perfect truth) to the prick of an ordinary needle; but the infliction of a punctured wound even of this simple kind may be a terror to children, and to nervous adults of both sexes.—*British Med. Journal, June* 9, 1860, *p.* 437.

12.—*The Treatment of Neuralgia by Powerful Currents of Electricity.*—In a paper read before the Academy of Medicine, M. Becquerel strongly recommends the treatment of neuralgia by this method, which has proved highly successful in his practice, statistics being furnished which show that nineteen cases of sciatica, and twenty-three cases of lumbar, intercostal, crural, and trifacial neuralgia, many of which were of old standing and not amenable to other remedies, were all perfectly cured by the judicious use of powerful electric currents.

The principle upon which this mode of treatment depends is as follows:—If intense currents of electricity are applied in the course of a nerve, its sensory power is benumbed for a time: an analogous effect is produced if an electric shock be communicated to a nerve which is the seat of neuralgic pain, the morbid sensibility becoming diminished and annihilated under the influence of strong currents repeated at rapid intervals.

Sometimes the disease disappears entirely after a single operation, but it more generally returns after a variable period of from a few hours to a day or two. As the electrisation is repeated, the recurrence of the neuralgia is always at greater intervals, and the pain becomes less intense, until, finally, a cure is completed, for the perfection of which from three to fifteen applications, each of two to five minutes' duration, are necessary.

In employing this treatment in cases of trifacial neuralgia great precaution is requisite, as otherwise, either cerebral congestion, or obstinate headache, never in themselves dangerous, but yet alarming and distressing to the patient may occur.—*Bulletin de l' Académie.*—*London Medical Journal, July* 1860, *p.* 48.

13.—ON CHRONIC ALCOHOLISM.

By Dr. WILLSHIRE.

[A good deal of discussion seems to be going on respecting the exhibition of alcohol in such cases as follow. Both sides are right. Practice, after all, and a long list of successful cases, induce the older physicians to bear two things in mind in these cases: 1. When a patient has an attack of delirium tremens, or is threatened with one, in consequence of drinking for a week or two to excess, it is best to withdraw all stimulants, and not to give opium—the case will right itself. 2. When a patient has for years almost lived on alcohol, and it is now from some circumstances withdrawn, it will most likely end in delirium tremens. The first case is caused by drinking alcohol, the second by suddenly withdrawing it. The two causes are quite different; consequently in the second case we must carefully give small doses of alcohol and as much nutrition as the stomach will digest. We think that those who oppose this opinion will some day be obliged to alter their views. The following cases would seem to contradict us, but all we maintain is that, although you may do without stimulants in

many cases, and all the better if you can, yet in many other cases you will lose the patient if you follow this out strictly.]

The notes of the following case were taken by Mr. C. Bennett, clinical clerk, Charing Cross Hospital.

Edward S., aged thirty-six, well educated, now an attorney's clerk, but formerly in India and China as a soldier for ten years. He there had dysentery twice, and what he supposes cholera. When abroad he drank very hard; indeed, the fleas and insects annoyed the soldiers so much that they were obliged to get drunk to enable them to go to sleep. At his period of greatest drinking he took, perhaps, one quart of spirits in the day, besides any other thing he might happen to get hold of; has seen some of his comrades drink a pint of spirits before going to bed; sometimes they were found dead in the morning; has seen some of his officers drink a pint of spirits in the afternoon. He had himself sleepless attacks, with delusions, when in India; but he maintains he never had delirium tremens. In 1849 he came to London; still drank pretty hard; was taken ill at Croydon, when his medical attendant stopped his drink. His friends had a physician from a well-known London hospital to see him, who said the medical man was killing him, and ordered him brandy-and-water. Two years ago he had typhus fever. He was then drinking hard, chiefly gin, and rum in his tea of a morning; at eleven he took some more. He next took stout at his dinner. In the afternoon he again took some gin or rum; and in the evening he had some porter, with, what he calls, a stiff tumbler of grog. He was then, he states, well for three months. He then began to feel very ill and wretched; his hand was unsteady; was obliged to resign his duties. Having no provision for his family, he got some law reporting to do for the cheap journals. At last, however, he could not write; he could not eat; he had bile in the morning, and constant belching; the bowels were rather irritable; he got yet more shaky; required more stimulants; his face became bloated; began to waste about the legs; had involuntary seminal emissions without erections. At length a large patch of eczema impetiginodes broke out upon the right hand and forearm. He now came to the hospital (Oct. 18th, 1859) upon the advice of a medical man and a lawyer's clerk whom he knew. He trembled a good deal, had a coated tongue, congested eyes, and was (as he has since informed Dr. Willshire) scarcely able to get into the room from debility and nervousness; and that when he was told to sit down he was glad, as he could not have stood a minute longer. He had been drinking latterly just as much as his means would allow him to procure; he was firmly impressed with the idea that he must have spirits, and when told that he must not have a drop, he fancied he must die. Dr. Golding retained him for two or three days after he came into the hospital, and then transferred him to the care of Dr. Willshire. All fermented fluids were strictly forbidden, full diet given him, and a mixture of

carbonate of ammonia for a few days. Poultices, lime-water, powdered calamine, and zinc ointment were used to the hand. All the skin came off like a glove from the hand and fingers, and, by the 10th of November, the hand was almost well; he had gained flesh, all trembling had left him, had plenty of confidence in himself, and felt altogether a different being. Latterly he has been taking quinine and sulphate of zinc, with tincture of valerian, purgatives, and as much as he likes to eat.

Nov. 17th. Improves; complains of not sleeping, however, since he has left off the morphia pills; hand and arm comparatively quite well.—*Lancet, Sept.* 8, 1860, *p.* 237.

14.—*Chronic Alcoholism, with impending Delirium Tremens, treated by Suspension of the Stimulus.* (Under the care of Mr. CANTON.)—For the notes of this case we are indebted to Mr. J. Reeve Shorto, late house-surgeon to the hospital.

James W——, aged thirty-six, married, a general dealer, was admitted into the Charing-cross Hospital on Nov. 14th, 1859. With the exception of what is mentioned below, he has always enjoyed tolerable health. His habits, however, have been most intemperate, he being accustomed to take eight or ten glasses of gin daily, besides two or three pints of porter, and three-quarters of an ounce of tobacco for smoking. He states that he lives well, and takes meat daily, excepting when he more than usually exceeds in drinking, when for a week or so he is unable to take any solid food, but confines himself to slops, broth, &c. At these periods also he is exceedingly nervous. About two years ago he was admitted into King's College Hospital with idiopathic delirium tremens. He was there treated with brandy, beer, and opium. He remained in the hospital twelve days. Nine months afterwards he had another attack, and six months ago a third, for both of which he remained at home, and treated himself by freely taking spirits and beer.

Nov. 14th. For the past week he has been, as he denominates it, "out on the drink." During the whole of to-day he has been indulging, and he has taken seven glasses of hot rum, besides gin and beer, and incessantly smoking. He has taken no solid food to-day. Towards evening he got into a row, and received a wound over the temple, which bled rather profusely. He applied at the Charing-cross Hospital for this, and, his wound being dressed, he was admitted an in-patient under Mr. Canton, who prescribed an opiate. In appearance he is the very picture of a man who is addicted to intemperance. His face is bloated; his eyes are expressionless, and have a peculiar unsettled stare; his hand trembles very much, as also his tongue (which is coated white), and his whole aspect betokens the enfeebled constitution of a confirmed drunkard.

15th. Very feeble. He has obtained no sleep. He spontaneously

assures the house-surgeon that he apprehends another attack of delirium tremens if he is not allowed some spirits; and when told that it was intended that none should be given him, he was much astonished, and feared he would not recover. Ordered ammonia and bark, with chloric ether and opium, every four hours, and, as his stomach was irritable, some beef-tea; to have an opiate at bedtime.

16th. Better. He has had some sleep. He is not so nervous, and is more tranquil in his mind. To have full meat diet daily, but no beer or spirits. Ordered a grain and a half of quinine, twenty minims of chloric ether, and fifteen of tincture of opium in water every four hours.

17th. Feels much better. He says he experiences the same sort of relief from taking his quinine mixture as he usually does from imbibing his dram of spirits.

21st. Still improving. The wound in the temple is almost well. The nervousness he complained of has almost departed.

A few days afterwards he left the hospital cured.—*Lancet, Sept.* 8, 1860, *p.* 237.

15.—*Three Cases of Delirium Tremens, treated without Stimulants; Fatal Result in Two.* (Under the care of Dr. ALDERSON and Dr. SIBSON.)

In the annexed cases no alcoholic stimulants were given after the patients were admitted into the hospital, as will be seen by the details, but some one of the preparations of opium, combined with ammonia, instead. In two a fatal result ensued, probably mainly brought about by the mechanical restraint employed. The prohibition of this terrible source of exhaustion cannot be too stringently enforced; for, as was recently observed in some clinical remarks on this disease made by Dr. Brinton at the Royal Free Hospital, "no matter to what degree reason may be degraded or dethroned, the delirious patient always seems sensible of the debasement and brutality implied by straps and bands." (Lancet, vol. i., 1860, p. 592.) If attendants cannot be procured to gently restrain and watch the patient, it were better under such circumstances to administer chloroform, which we have known to do more good than all the strait waistcoats; and we have recorded cases which, sleep having been induced by its employment in the London Hospital, terminated in recovery. (Lancet, vol. ii., 1858, p. 449.) The notes of the subjoined cases were kindly furnished by Dr. W. H. Broadbent, medical registrar to the hospital.

Robert S——, aged forty-five. butler, was admitted into St. Mary's hospital, under the care of Dr. Alderson, May 16th. He had been engaged in his occupation as butler up to the date of his admission, enjoying good health, but drinking largely. When brought to the hospital, his countenance had a wild expression, there was general tremor, and he was apprehensive of some intention to injure him on

the part of his fellow-servants; the skin was perspiring; pulse quick; tongue white and creamy. He was ordered two grains of opium at once, and a grain every hour till sleep should be induced.

May 17th. In much the same condition; can answer a question rationally, but is unable to fix the attention on any subject. The following mixture was substituted for the crude opium: sesquicarbonate of ammonia, four grains; solution of morphia, twenty minims; tincture of lavender, fifteen minims; and acetate of ammonia mixture, one ounce, every four hours.—Eleven p.m.: Very noisy; sweating profusely; in other respects about the same. As it was found that he and another patient suffering from phrenitis were mutually exciting each other, and disturbing the ward by responding in loud shouts, he was removed to a separate ward. During the night he became violent, and the attendant, without consulting the house-surgeon, put on the strait waistcoat. At four a.m. the house-surgeon was called. His struggles had suddenly ceased, and he was found unconscious; pupils fixed and dilated; surface cold and clammy; slight convulsive movements of the extremities, and twitching of the mouth; pulse imperceptible; respiration suspirious. He died in ten minutes.

At the post-mortem examination nothing was found beyond slight sub-arachnoid effusion.

Thomas C——, aged thirty-six, admitted, under the care of Dr. Alderson, June 4th, 1860. He is a pipe-maker, and, in carrying round his pipes to public-houses, has been in the habit of drinking much gin and beer. He has had several attacks of delirium tremens. When admitted, had not slept for two or three nights, but had imagined that people were jumping about outside his window, and deriding him. Hand and tongue unsteady; skin cool; pulse 88; tongue white and moist; no actual delirium. Ordered two ounces of compound senna mixture, to be followed by twenty minims of solution of muriate of morphia, with three grains of sesquicarbonate of ammonia in camphor mixture, every four hours. He had a pretty good night, continued to improve, and on the 12th was discharged cured.

Thomas M——, aged thirty-four, admitted, under the care of Dr. Sibson, March 16th, 1860. For the last six months has been suffering from cough, with pain in the side and expectoration, and he has been in the habit of taking half a pint at least of brandy daily. When admitted, he was agitated, tremulous, apprehensive, but not delirious. Skin moist; pulse 90; tongue white and moist. He was ordered ten grains of colocynth pill with calomel, and mixture of the acetate of ammonia, with excess of ammonia; at night he had forty minims of Battley's solution, and twenty minims every three hours till sleep was produced. Diet, broth, with a pint of porter.

March 17th. He had a little sleep in the night, and seemed about the same. The opium to be repeated at night.

18th. Did not sleep, and now insists on going home, imagining

that some mischief to him was intended. He was easily persuaded to return to bed; but he required constant watching during the day, and towards evening became violent. Battley's solution to be given every two hours.

19th. Very much excited, and with difficulty kept in bed; imagines that he is to be punished for some crime. Hands tremulous; skin perspiring freely; pulse 98; respiration 24. As he became more and more violent, and as he was an exceedingly powerful man, and could not be restrained, he was removed to a separate ward, and the strait waistcoat was put on. His efforts to free himself were fearful, and though the opium was increased in quantity, and given at shorter intervals, he had no sleep.

At about six a.m. on the 20th he ceased to struggle, and seemed to sleep; but it was soon seen that this was not sleep, and before the house-surgeon could be called he was dead.

On post-mortem examination, both lungs were found universally adherent and greatly congested. There was slight subarachnoid effusion, and both lateral ventricles of the brain were greatly distended by fluid.—*Lancet, Sept.* 8, 1860, *p.* 238.

16.—ON DELIRIUM TREMENS.

By Dr. JEFFERY A. MARSTON, Assistant-Surgeon, Royal Artillery, Malta.

[The following paper by Dr. Marston is a most sensible one, and they who are now treating all cases of delirium tremens without alcohol will do well to consider it. Few are more strenuous advocates of total abstinence from all stimulants than ourselves in cases of health, but in certain diseases we have found it indispensable to carry the patient over certain periods of exhaustion. Dr. Marston says:]

First of all, let us see that our terms are precise and clear. I would say that there are three separate and distinct forms of the disease in question: that if any one plan of treatment be pursued in all, and if, without reference to their distinctive features, they be individually and severally heaped together under one head, we can obtain no reliable data. Not a little misconception, it seems to me, has arisen from this very source.

I. Delirium è potu, or Delirium Ebrietatis properly so called.

II. Delirium Ebriosorum.

III. Delirium complicated with { Renal, Hepatic, Gastro-Enteric, Cardiac, Central } Diseases.

Professor Todd, in one of his clinical lectures, has some admirable remarks upon the two first forms, and their distinctions. The first I

would illustrate thus:—It happens in the younger and more acute drunkards (if I may so term them). The disease follows quickly after a debauch—within 24 or 48 hours. The symptoms are—The tongue very foul and tremulous; great headache; face rather flushed; tenderness often upon pressure of the epigastrium; nausea; sometimes vomiting; perfect anorexia; sleeplessness; tremors; hallucinations; illusions; restlessness; an excited manner; a quick, soft, and tolerably full pulse; and often there is present a smell of spirit. Now, these cases are by far the most frequent. The disease occurs in a man whose means prevent a regular steady soaking, but in one who drinks very hard whenever his pocket allows it. However frequently this may be, there is always a good and distinct interval—weeks or months. The subject of the disease goes in for a heavy night or so at a time—his money is exhausted—he does or does not go to prison, but at any rate he does not drink again for some period, for the best of all reasons—want of means. Here is the ordinary form of the disorder: An acute alcoholism—the drink being *in* the man. An emetic-purgative, with quiet and repose for two or three days, sets him all right again.

The second form is a delirium of drunkards, in contradistinction to a delirium from drink. The illustration of this form will be:—The subjects of it are older—have the outward and visible signs, and bring the history of a habit of drinking; hard drinking indeed, if the aggregate be looked to—the steady weekly consumption of spirits, to wit—but less hard than the first variety in a given space of time. Those men have kept out of the guard-room, and are shrewd enough to keep aloof from their officers; bear a good regimental character; are seldom, if ever, in hospital; and although long suspected in the regiment of being secret drunkards, yet are only proved so by some accident—as turning out at night to a fire—or admission into hospital for some trivial disease. At last he is caught, and lodged in the guard room; or by some means or other he is with all suddenness deprived of his drink; and delirium tremens sets in, appearing from the second to the seventh day after confinement, while perchance the man is awaiting his court-martial.

In short, you get the history of a man who has drank for years and years; during which time he has performed his duty under sharp supervision, and has not suffered from any disease. He is deprived suddenly of his stimulus, and takes delirium tremens.

Complicated Form.—This variety will take in the various symptoms of any organic disease present, and complicating the case. It would be impossible and needless to enumerate the various complications, further than to remark that their recognition and diagnosis is all-important; the difficulty of their treatment very great; and that the mortality far exceeds that of the other and simpler forms of the disease.

The visceral or glandular derangements become evident for the first time during the attack of delirium tremens—the man never having

been before in hospital,—and are then only arrived at, from the fact that some unusual symptoms, as convulsions, jaundice, œdema, persistence and peculiarity of the delirium, or albuminous urine, make them apparent.

The treatment of such cases must necessarily be modified according to the diseased state and its indications.

Having premised this much, let us turn to the points in dispute, which appear to be: That the theory of the causation or etiology of the disease hitherto propounded is wrong, its pathology wrongly stated, the indications for its treatment misunderstood, and the special modes of treatment themselves (particularly that by opium and stimulants) have been erroneously and injuriously pursued.

It is often better to watch than to correct, and I would ask how many cases classed under the first form have been enumerated with the second and third varieties? I fancy that by far the larger number of the so-called statistics consists of individual cases of the first form, and if so, the generalization from them cannot fail to be vitiated, when applied to the whole disease.

Dr. Watson expressly says, that some cases occur after a long debauch, and others in which the patient has not abstained, but is continually fuddled; and here the disease arises because the man goes from his ordinary positive to the comparative degree.

There is no doubt that those cases are numerous, and their plain inference is "poisoning;" but so much has been made of them, as virtually to exclude the occurrence of other forms.

Etiology.—Taking the objections seriatim, the etiology; Watson says, the predisposing cause is drink; the exciting cause, the privation of it. Against this view there is urged the frequency of the disease following a debauch, &c., without any privation; in short, the frequency of the first form. Such frequency, it is argued, proves the disease to be the result of poisoning, and the sufficiency of the poison alcohol to form at once the exciting and predisposing cause. The occurrence of some disease, such as catarrh, influenza, gastric disturbance, in a drunkard, may be also an exciting cause. Lastly, the statistics of prisons are against the theory, it is said, of privation. The first objection is shortly this, that we have proof of one cause (toxæmia from alcohol) being sufficient, and hence it is unphilosophical to seek a second. I can only meet this by stating, that there are different forms of the disease, which may have different causes, nay, a plurality of causes. Besides, I shall adduce positive evidence of the disease following the withdrawal of alcohol. With regard to prisons, I can only speak of military ones. No cases occur in them, I am aware, and am not surprised at the fact; but they occur antecedently, coming on in the guard room, where, if a man be tried, he awaits his court-martial, probably for many days; and I state as a fact, that cases of delirium tremens commonly occur from the first even to the seventh day after the privation of liquor. The guard-room statistics

would hence be every bit as strong the other way. I am aware that statistics do not settle the fact, they do not prove cause and effect, viz., deprivation of accustomed stimuli as a cause of delirium tremens; but they cannot certainly be urged against it, for they support at least the view of this privation being frequently an antecedent to the appearance of the disease. Is it a necessary antecedent? I believe it is in some cases. Take the following:—

Sergeant D., æt. 39 years,—in daily expectation of a good-conduct medal.—Admitted April 13, suffering from a small boil upon the lobular appendage of the right ear; in perfect health apparently. Upon the afternoon of the 14th I noticed that he was very tremulous and nervous, and asked him, privately, about his habits. He denied, in the most positive and awful terms, that he drank hard, and refused my offer of a glass of brandy. Upon the morning of the 15th, I learnt that he had slept badly, and found him suffering from a decided attack of delirium tremens. He talked incessantly, was bathed in perspiration, had illusions, and fancied he saw strange animals, and heard strange noises. His tongue was coated, and very tremulous; the eyes ferrety, the pupils moderately contracted. He was ordered a sharp purgative, and cold douche to the head, followed by a basin of beef-tea. At 12 noon he took ℥i of laudanum, but was sick, vomiting nearly it all; at 2 p.m. he had some warm brandy and water, with ℥ss of laudanum, which he kept down; about 3 p.m. he had more beef-tea, and said he thought he should sleep, lying down for that purpose; I happened to be in the ward at the moment, and my attention was arrested by his livid face and heavy breathing. In about a quarter of an hour, he had an epileptiform convulsion; two or three followed after a short interval, and in about three-quarters of an hour he died with symptoms of apnœa. Besides cold douche, artificial respiration by Marshall Hall's method, and enemata of brandy, were tried without avail. The *post-mortem* was made most carefully by myself, and I could detect no organic disease to account for his death. The left ventricle of the heart, however, was in an advanced degree of fatty degeneration; and, besides some venous congestion of the membranes of the brain, there were a few drachms of fluid in the cerebral ventricles.

Now, I would remark how strong is the tendency of the evidence, negative and positive, of the facts here. A man is entitled to a good-conduct medal—which presupposes eighteen years' absence from the defaulters' book as regards courts-martial. I learnt from his wife, subsequently, that for six or eight years he had drank very hard, and that, although she had never seen him drunk, yet she could not say that he was ever perfectly sober any night; that she did not remember his having ever been in hospital, or suffering from any disease. He comes into hospital, is deprived of his liquor directly, and in fifty-six hours is dead. This appears to me a strong case, but it is not by

any means an isolated one; and others equally positive will be cited, far more than sufficient to meet the logical requirements of one grain of *positive* against a bushel of *negative* evidence.

Pathology.—Dr. Watson's views may be epitomised thus:—The disease is "Exhaustion with nervous irritation;" the remedy, "sleep." Against this is urged the toxæmic view, and the positive chemical evidence of the presence of alcohol in the cerebral ventricles, and that sleep is an *effect* and sign of the improvement in the disease, and not the cause of it.

Dr. Watson uses his terms advisedly. He does not pass them for more than they are worth. Are there not certain acquired physiological conditions or states of system which produce uncontrollable cravings and desires? and are not these cravings instinctive desires of a need felt by such an abnormal system? and supposing them unyielded to, do we not get nervous exhaustion and depression? Is it true that horses fed upon arsenic fall into a bad state of health, when its exhibition is discontinued? The facts related of the Styrian and Hungarian peasantry relative to their frequent use of arsenic, and the evils which almost invariably arise from its discontinuance; the almost universally spread instinct in man to the use of narcotics, stimulants, &c. (such as opium, betel nut, tobacco, cocoa, with the rest); the growth of the custom into a habit, and the way in which men are impelled to the continuance of that habit, from the fear of the chain of morbid phenomena which follow the cessation of it;—do not these run very parallel to the facts we observe from the use and abuse of alcohol? Chossat's experiments upon the effect of starvation on animals would indicate that the nervous tissues undergo remarkably little relative loss compared with the other tissues, in spite of the great quantity of fat they contain, and their almost fluid consistence. This has been held to explain the cause of the curious psychical phenomena preceding death by starvation. Restlessness, delirium, and prolonged sleeplessness, are common precursors of death in such cases. Now it will be said, that no analogy can be established between such cases and the same phenomena following the withdrawal of alcohol. But I am not convinced of that. The whole doctrine of diets, however satisfactorily it may be settled upon a chemical basis, is decidedly not settled upon equally certain physiological data. We have facts in abundance to prove that the chemical value of food is not the physiological one, and that both man and animals live and grow upon substances, and in proportions, perfectly different from what a chemical view would indicate, or conceive possible. When we observe how spirit is meat and drink to a drunkard; how his system affords the proof of an altered and abnormal nutrition; how, in short, he has a special physiology of his own, it seems to me a natural and rational consequence, that the poison to him is no poison, but, on the contrary, a special fuel for his nutrition and development,—albeit diseased. Liebig has shown how the chemico-physiological theory of the action

of stimulants upon the human system, is in accord with the actual experience of landlords and others, viz., that a far larger amount of food is consumed by the abstainer from alcoholic fluids, than by one who partakes a moderate quantity of them. It seems to be a well-ascertained fact, that alcohol economises the food and tissues, by arresting the amount of secondary metamorphosis. Then we have the influence of the custom producing a habit, and its known effects upon the body. Can any habit be suddenly discontinued and broken without some, nay, even a grave effect upon the nervous system?

There seems to me to be no end of evidence to prove that the sudden curtailment or withdrawal of any habit may produce nervous exhaustion; and if so, why, irritability is a necessary concomitant and index of that condition!

In regard to sleep. A drunken sailor knows very well that if he can "sleep it off," it is the best and most natural way of terminating his fit. No doubt the tendency to sleep is a sign of improvement in a case of delirium tremens. Dr. Watson and others urge that sleep is the necessary precursor of improvement; but if I understand the objection raised to this view aright, it amounts to this, that before a patient recovers from delirium tremens, and as a sign and effect of his improvement, he sleeps—very probable; but I am sure the *post hoc* is often a *propter hoc*, viz., not that he sleeps after he improves, but he improves because he has slept.

Case.—G. S., aged 30, admitted June 8, 1858, from the guard-room, where he had been confined two days. Had been for years a hard drinker. It was stated (but not upon reliable evidence), that prior to enlistment he had been confined for a few months in a lunatic asylum. Upon inquiry, it was found that he had been drinking very hard for some days, and that the debauch terminated about three days prior to his admission into hospital. He was labouring under all the symptoms of unmistakeable delirium tremens. After the administration of a purgative and a saline antimonial mixture for twenty-four hours, without any amendment, opium in grain doses was commenced, and continued until his pupils became somewhat contracted, when it was omitted. He was allowed milk diet, with a basin of soup at bed-time. Forty-eight hours having elapsed in hospital without any sleep, and his delirium, tremor, and symptoms of exhaustion augmenting, it was determined to give him chloroform, for which purpose another assistant-surgeon with myself, alternately, sat at his bedside all night: and he was kept, at intervals, under its influence for eight hours, during the greater part of which time he slept soundly. At the expiration of this period he awoke, and partook of some broth. He appeared far less tremulous, had lost his rapid delirious conversation, but retained his suspicious manner, and was evidently haunted by illusions. After some interval he was again put under the influence of chloroform, and slept for four hours deeply. Awaking, he was still

more rational and restored; and after drinking two bottles of porter, he spontaneously fell asleep. In this state he continued six hours, and awoke rational and well. He was retained in the hospital for some period, on account of some dyspeptic symptoms and boils, and with the view to invaliding on account of his uncontrollable habit of drinking. This man was a dipsomaniac indeed. I think I never beheld features so expressive of a true drunkard in my life. During his fits he seemed to have laboured under certain dominating passions, and these had left such impressions upon the facial muscles as to have permanently altered his whole physiognomy.

Now, here we have a case of delirium occurring in a chronic drunkard, who had been deprived of his stimulus for two days. He is admitted into hospital, and does not sleep, his symptoms becoming worse; by the aid of chloroform he slept for six or eight hours—improves—and by the administration of more chloroform with the aid of porter, he sleeps again, and is cured.

A. B., a mess sergeant in a line regiment, had always been suspected of drunken habits. One day he absconded with some money, was caught, and confined to the guard-room. Whilst there, he became the subject of delirium tremens, and was brought to hospital, with the history of having endeavoured to poison himself with arsenic. This case was a very severe one, for the patient had an ever-present sense of his crime, a fear of punishment, and was, moreover, suffering from all the symptoms of the disease. Numerous means were used (including opium) without any benefit, and a fatal prognosis was formed of his case. The regimental surgeon administered chloroform by inhalation, and procured artificial sleep for many hours. The man awoke so much improved, that its administration was recommenced, and he was quite restored by its aid.

Here we have the symptoms continuing for a certain period without any improvement, while induced artificial sleep or narcotism was attended with such marked improvement, as to have impressed the medical attendant with the belief that his patient owed his life to the chloroform.

It would be tedious to cite other cases proving the same thing. I conclude from them, that to procure sleep is sound, excellent advice, the good effects of which are borne out by actual experience.

Treatment.—The use of stimulants (alcoholic) in cases of delirium tremens, with the view of removing the exhaustion, allaying the morbid irritation, and procuring sleep, would be looked upon as even more wrongly directed. If the etiology usually propounded be radically and totally erroneous, then we are guilty of adding more poison to an already poisoned blood, to procure what is not needed, and what the presence of alcohol in the system is preventing—sleep.

G. M., aged 45, but grey, and looking much older, was admitted into hospital for some gastric disorder. This man was, and had for

years been, a very hard drinker, and his features, particularly the nose, indicated "potations deep." One morning (after he had been in hospital for some days) I was called to him. He appeared insensible, was breathing very heavily, the face and lips dark and turgid, the pupils contracted. Whilst examining him he had a convulsion of a tetanic character, the body being arched in the position of opisthotonos. The muscles of the fore-arm were so tense that the radial pulse could not be felt. The heart's impulse was scarcely perceptible. Having thrown a bucket of cold water over the head, I took advantage of a few moments of apparent consciousness to pour a glass of hot brandy and water into his stomach; and he recovered almost immediately, so much so, that the medical officer, whose patient he was, could scarcely credit the state in which I found him.

G. P., aged 36, admitted April 14, 1858, from the guard-room, where he had been confined two days. His disease was unequivocally delirium tremens, and no remedial measure seemed to benefit him. He was tremulous, restless, delirious, and did not sleep even with the aid of the fresh administration of opiates. Two bottles of porter were given him at bed-time, and he was observed to smoke during the day some strong tobacco. His hands were so tremulous that the orderly had to hold the pipe in his mouth. By these means he slept, and slept well, and made a good recovery, after having been dosed unavailingly for four days previously to procure that result.

At this moment I have a soldier in hospital who has always drank freely. He has been under treatment upwards of eight days for trivial bronchitis, and symptoms of incipient delirium tremens appearing now, necessitate the use of alcohol and opium with manifest advantage to the man, as regards sleep, appetite, and the disease itself.

These cases illustrate two points: the actual occurrence of delirium tremens after and during the privation of liquor; and the speedy removal of the symptoms by the re-establishment of the custom, when other means failed.

I have purposely selected these cases, because they illustrate also the fact, that whilst the men were taking their accustomed stimuli, they continued well, for they were men who never appeared at hospital at all.

I shall not give cases of apparent cure by the administration of opium, as they can be found in any work; and, after all, they are no proof that your patient recovered by the treatment, but, perhaps it will be urged, in spite of it.

The most curious cases are those in which the delirium continues for a long period, but in a modified degree; the patient sleeping tolerably every night, eating and drinking, and performing all his functions well. It is well to look out here for some complication (particularly renal or hepatic disease), for it is surprising how small an amount of urea circulating in the system may give rise to a persistence

of anomalous symptoms. Besides the uræmic, we have a peculiar and difficultly treated form of the disease when jaundice is present, whether arising from fatty degeneration, cirrhosis, or other hepatic disease. These cases, of course, are more frequently fatal; but I find that, after local depletion, purging, or diuresis, stimulants, more particularly gin, are not only not contra-indicated, but are decidedly useful, more particularly if the patient be an old chronic drunkard.

The most fatal form by far is that in which we have delirium tremens occurring in a person already the subject of typhoid fever—cases by no means uncommon in this climate. Having separated, however, these cases, there will remain many in which a chronic derangement of the nervous system is manifested, the patient sleeping night after night, for longer or shorter intervals, and performing all his functions well.

In a few cases, opium given in full doses at bed-time will secure a deep sleep and manifest improvement. In others, a liberal but regulated allowance of stimulants will prove advantageous, when everything else has been tried in vain. In some, no plan of treatment will succeed, although the patients frequently recover after a long interval, while others lapse into chronic mania, melancholia, or some form of insanity, ending their days in a lunatic asylum.

With reference to that singular phase of our mental life-sleep, Sir Henry Holland advances views, which my observation of the sleep of delirium tremens patients has led me to think perfectly truthful.

He is of opinion that sleep is not a unity of state, but a series and succession of states, ever varying from moment to moment. These variations having every degree of diversity, from complete wakefulness to the most perfect sleep of which we have cognisance. It has long occurred to me that the sleep of drunkards and in delirium tremens differs much from the normal standard of intensity. Every one must have experienced in his own person, when he was anxious to awake at a certain hour, how he awakes at that time with a feeling that he has not slept well, or at all, although he may be assured that he has slept very soundly.

Sir H. Holland's observations are so good upon sleep and dreaming, in relation to delirium and insanity, that I shall quote his words:—"I know of no principle so capable of affording a guide, as that which views all the forms of insanity, including delirium, in their relation to corresponding healthy states of mind, tracing this connection through those intermediate grades, which are so numerously exposed to us in the various conditions of human existence. The diversities of mind in what is accounted its healthy state, the effect of passions in suddenly altering its whole condition, of slighter emotions in gradually changing it, and of other incidents of life in affecting one or more particular faculties; its subjection periodically to sleep, and casually to the states of intoxication, somnambulism, and reverie; its gradual transition in fever from a state where there is consciousness of vague and

wandering ideas to the state of perfect delirium;—all these furnish so many passages through which we may follow sanity into insanity, and connect the different forms of disordered intellect as well with each other as with the more natural and healthy functions of the mind."

To sum up, I would say that the first and most frequent form among soldiers of this disease requires rarely indeed opium, particularly at the commencement of the attack; in short, no specific treatment is necessary. In the second variety, I would give it cautiously in moderate doses, after free purgation, provided I did not find my patient improving by rest and tranquillity. The opium had better be given at any rate in a full dose before the accustomed hour of sleep. Should it not succeed, my experience would indicate a "hair of the dog that bit him," in the shape of porter or hot brandy and water, spite of what has been urged to the contrary. If the surgeon avoid both opium and stimulants, and his patient goes on badly, depend upon it the chances are in favour of another doctor advising one or both of these noxious agents, with much advantage to the patient, to the no little chagrin of the first medical attendant. Where great irritability of the stomach is present, there is nothing better than a sedative dose of calomel (gr. vj.), with or without opium, and a large enema.

Supposing the physiological effects of opium upon the system to be manifested, without sleep or improvement following, I should omit its use for some hours, give my patient some good broth, flavoured with brandy or wine, and induce artificial sleep at night by the aid of chloroform.

I trust it will be seen that the use of opium is advocated as a measure requiring discrimination and caution, but as a reliable one in many instances.

Of course the complicated forms require that the greatest discretion should be exercised in its administration. Where an embarrassed circulation exists, marked by venous congestion of the mucous membranes and duskiness of the face, it is better avoided altogether. Pulmonary emphysema, if extensive, cardiac disease, or indeed any thoracic complication, will require also great care, if they do not indeed prohibit opium in any form.—*Edinburgh Monthly Journal, Oct.* 1860, *p.* 306.

17.—TREATMENT OF DELIRIUM TREMENS BY DIGITALIS.

By G. M. JONES, Esq., Dr. BALLARD, Dr. WILLIAMS, and Dr. WEBSTER.

[Mr. JONES, of Jersey, treats these cases on a novel plan. His opinion is always valuable. From most other people we should dissent if they gave such an opinion, but Mr. Jones is truly trustworthy. He was led to give digitalis from mere accident. He says:]

As to the dose, experience has taught me that the best dose is *half-an-ounce* of the tincture given in a little water. In some few cases, this one dose is enough, but generally a second dose is required four hours after the first. In some cases, but very seldom, a third dose is called for; but this hardly ever need exceed two drachms. The largest quantity I have ever given was *half-an-ounce* at first, *half-an-ounce* four hours afterwards, and another *half-ounce* six hours after that—making an ounce and a-half in ten hours.

As to the effects of these doses, my impression is that the action is on the brain, not on the heart. The pulse, so far from being lowered in force, becomes fuller, and stronger, and more regular, soon after the first dose. The cold clammy perspirations pass off, and the skin becomes warmer. As soon as the remedy produces its full effect, sleep for five, six, or seven hours commonly follows; sleep is the guide as to the repetition of the dose. No action on the kidneys is evidenced by any unusual secretion of urine. Sometimes the bowels are slightly acted on, but not commonly. I have never once seen any alarming symptom follow the use of these large doses of digitalis. The only case I have lost since adopting this treatment had a tumour in the brain. In three only was other treatment adopted after digitalis had failed to procure sleep; in other words, in sixty-seven out of seventy cases digitalis was the only medicine used, and sixty-six of these patients recovered. I do not mean that these are the exact numbers of those treated; I am certain as to the death, but I may have had more recoveries. I am well within bounds in saying seventy cases in twelve years, and that all of them were well-marked cases of delirium tremens. Slight cases of nervous derangement after drinking I have seen in great numbers; but I speak here only of such cases as required active treatment. My previous experience of the results of the treatment by opium, or some of its preparations, by anti-spasmodics, &c., had certainly been much less successful; the proportion of deaths was larger, and the recovery much less rapid. Again; I have treated more than one patient successfully by digitalis, who, in subsequent attacks elsewhere, has been treated by opium and died; and in many of the cases in which I have used digitalis successfully, opium had been previously given without any good effect.

I will only allude to one case in illustration:—On September 9th, 1860, I was called to see a gentleman, 48 years of age, who was in a very alarming state, having been without sleep four days and nights, having been "muddled" for two months before, and having previously had "fits of the horrors." He had been treated by another practitioner by opium in moderate doses, but had become worse, and when I was sent for it was the opinion of Mr. Spencer Wells and Mr. McCrea—who accompanied me in my first visit—that the case was as bad a one as *they* had ever seen; certainly *I* never saw a worse. The pulse was almost imperceptible; the skin covered with cold, clammy perspiration; the face deadly pale; the lips blue; the hands tremu-

lously grasping the air; the eye expressive of great fear; the mind gone; he was muttering incoherently. With some difficulty I passed half-an-ounce of tincture of digitalis down his throat in the presence of my friends. In a few minutes he became more tranquil, the pulse was felt more easily, and we left him. After four hours I found that he had not slept, but he was rather more sensible, less tremulous, and warmer. I accordingly repeated the dose. Three hours after that, as he had been still without sleep, though in other respects improving, I gave two drachms more, making ten drachms in seven hours. After this he had some sleep, and had slept at intervals during the night. The next morning Dr. Ballard saw him, with my other friends, and all of them were much pleased with the great improvement manifested. He was sensible, his fears had disappeared, he was very slightly tremulous; the skin was warm, the tongue moist, and the pulse full and regular at 90. The heart's sound and impulse were normal; the bowels had acted once, and urine had been passed in natural quantity. After this he took some broth, drank freely of imperial and lemonade, but took no stimuli of any kind, or any other medicine. He slept uninterruptedly for three hours and a-half in the afternoon, and at intervals in addition. The next night was a good one; and when he was seen by my friends again the next morning he was almost well, and calling out for a mutton-chop.

I trust that this narrative of the results of my experience may induce others to follow what I believe to be a very valuable practical lesson; but I must warn those who do so not to try, as I have done, any smaller doses than those I have recommended. They would not only lose valuable time by so doing, but I believe would do harm. Doses of half-a-drachm or a drachm do no good whatever; and the pulse, in some cases where I tried them, became intermitting. I have never seen this effect from the larger doses; on the contrary, a feeble intermitting pulse has generally soon become fuller and more regular, proving. I think, as I said before, and as I again wish to impress on the profession, that the curative action is on the nervous system primarily, and not on the organs of circulation.—*Med. Times and Gazette, Sept.* 29, 1860, *p.* 301.

[Dr. Ballard, of Islington, corroborates Mr. Jones's opinion, and states as follows what he saw in two other cases, one in the hospital, and another in private lodgings.]

The former was a man apparently about fifty years of age, the other a young man about twenty-five or twenty-six years. Both had been drinking hard up to the period of seizure. In both, the characteristic symptoms of the disease were clearly marked, the skin cold, and the pulse very feeble; in the hospital patient scarcely perceptible. In both cases, the earliest operation of the medicine was seen in the abatement of the nervous phenomena, in greater quietness of manner, and in the disappearance of delusion. Coincident with this the

warmth returned to the surface, the pulse resumed its natural fulness, and a healthy perspiration appeared upon the skin. After this improvement had proceeded to a certain extent, both patients slept, but I could not regard the sleep as anything but an index of the favourable progress of the case. The improvement in the general condition clearly did not depend upon its production; first, because marked amendment had preceded it, and, next, because it was not prolonged beyond a very few hours. In no one of the three cases was there any unnatural increase in the quantity of urine passed. My surprise at witnessing the success of this remarkable treatment was only equalled by my admiration of the boldness and courage of the man who could introduce so startling an innovation of our practice. It certainly is not what any one would have expected: that a feebly-acting heart would be roused into normal action by a medicine like digitalis; yet this one of its earliest effects in the dose given by Mr. Jones.

From time to time very daring doses of digitalis have been administered. Dr. Pereira, in his "Elements of Materia Medica," mentioned the practice of Mr. King, of Saxmundham, who was in the habit of giving, in acute inflammation, doses of the tincture amounting to half-an-ounce or an ounce, without the production of any dangerous symptoms. A pupil of Mr. King once informed me, however, that during this treatment the maintenance of the recumbent posture was rigidly enforced. In neither of the cases which I saw treated by Mr. Jones did any pains appear taken to secure this precaution. In truth, the action of the drug upon the circulation, when it is used in inflammatory disease and in delirium tremens, seems utterly different. The effect in the former case, according to Mr. King, is to subdue the pulse and to render it irregular, an operation confirmed by Dr. Pereira's own experience of large doses; in the latter, the pulse increases in force and fulness.

My own impression, from what I have seen and heard from Mr. Jones, is that in tincture of digitalis we have a true *counter-poison*,—an addition to that class of mutually-antidotal poisons of which opium and belladonna have been the most recent examples. Alcohol is the remedy by which we counteract the depression produced by digitalis: the converse is also true,—digitalis is most remarkably an antidote to alcoholism.

At a meeting of the Medical Society of London, May 12, 1828, Dr. Thos. Williams related an instance of a drunken butcher having taken two ounces of the tincture of digitalis in two doses of an ounce each, in quick succession, without the slightest inconvenience ensuing. This is quoted by Dr. Pereira.

When first I heard of Mr. Jones's treatment, I naturally suspected the quality of his tincture; but on mentioning this, he assured me that he had employed tincture from a variety of sources, and among the rest from Apothecaries' Hall, and always with the same result.—*Med. Times and Gazette, Oct.* 13, 1860, *p.* 363.

[This practice of Mr. Jones is recommended also by Dr. A. WYNN WILLIAMS, whose opinion is as follows.]

That such doses are as a rule not dangerous when there is any unnatural excitement of the nervous centres, is fully borne out by the fact that even ounce-doses of tincture of digitalis have been prescribed twenty years ago to ward off epileptic attacks, when there was any preliminary warning of their approach. I was under the impression that it was Dr. Billing who so ordered it, but I cannot now find that he did so in any of the editions of his works. In such cases I have been in the habit of prescribing it in half-ounce doses for many years, and have found it by far the most efficacious remedy.

The following is a foot-note, written by myself, many years ago, under the head of Epilepsy:—"I have certainly averted the impending paroxysms by administering very large doses of digitalis, which does not appear to be so unmanageable and uncertain in its effects in large doses as in continued small ones. I have given as much as half-an-ounce of the tincture on the threatened approach of a paroxysm, and never saw any ill effects arising from administering so large a dose. One patient always kept a draught of the above dose in the house."

There is one form of excitement of the brain which I have never had an opportunity of administering it in, namely, puerperal convulsions. In this formidable disease, I decidedly think it deserves a trial.—*Med. Times and Gazette, Oct.* 6, 1860, *p.* 342.

[Dr. WEBSTER, Physician to the Northampton Infirmary, although he would, he says, be timid in adopting Mr. Jones's treatment by digitalis, gives the opinion and practice of Mr. Wood, of Heene Worthing, who treated a case of this kind with large doses of digitalis. Dr. Webster says]

Corroborative testimony to the innocuousness of such a dose of our usual pharmacopœial preparation of that tincture, in our climate, to the average constitution of our patients, can from one or the other no doubt be supplied (Dr. Wynn Williams has given us one); and I would try to report a somewhat analogous case, mentioned to me by Mr. F. B. Wood, of Heene, Worthing, and until recently practising in this town, hoping that others may thereby be induced to furnish their quota of experience *pro* or *con*. Mr. Wood writes to me as follows:—

"In reference to a plan recently suggested in the *Medical Times and Gazette*, by Mr. Jones, of Jersey, of treating delirium tremens by tincture of digitalis, in compliance with your wish I send you a narrative of the case, in which (many years ago) I treated a patient labouring under furious delirium, with, as I then thought, large doses of the same drug. I should commence by stating that I had never

previously attended the patient, but had learnt from reliable authority, viz., his previous medical attendant, that the patient, in the six or seven preceding years had three or four similar violent attacks. The treatment then adopted had been—venesection pleno rivo from both arms at once, and frequently repeated; hundreds of leeches to the forehead, by twenties, and twenty-fours at a time; large quantities of calomel, and long-continued drastic purgation. There were also several, and severe, relapses on each of these occasions; and in one instance, after an interval of three weeks from the first seizure. On the last two occasions, when convalescent, the patient was in such a low and melancholy state, that daily and nightly watching, for three or four weeks, had to be maintained, for fear of the commission of suicide. I was summoned to attend at the last attack, in October, 1844 or 1845, and the residence being several miles away from Northampton—and having been forewarned of the probable nature of the case—I went armed with two ounces of the tincture of digitalis. Upon my arrival I at once gave a tea-spoonful of the tincture, and repeated it every hour for four doses (the second dose having been given in rather less time). The patient then becoming much quieter, and circulation steadier, I reduced the dose to half-a-drachm every two, three, and at last four hours, during the remainder of the first twenty-four hours from the seizure. The patient had only two or three slight relapses, and these within the first four days. On the third day, when there was a slight relapse, the physician who had previously attended was summoned in consultation. He applied twelve leeches to the temples and forehead, continued the digitalis, and ordered a fair quantity of calomel and colocynth. This was all the blood withdrawn on this occasion, and I confess even that against my judgment. However, when convalescence was established, there were none of the old symptoms of debility, melancholy, &c., as in previous attacks. Altogether the patient took about fourteen drachms of the tincture of digitalis, in the first hundred hours of his illness."

As this patient has been under my supervision from time to time since Mr. Wood left this town, I can corroborate the fact of there having been no return of the complaint, even though there has too often been a sufficiently adequate amount of the accustomed provocation.—*Med. Times and Gazette, Oct.* 13, 1860, *p.* 364.

18.—*On the Treatment of Alcoholism.* By Dr. SMIRNOFF.—Dr. Smirnoff states that he has become convinced by repeated trials that the *asarum Europœum* well deserves the reputation it has obtained in Russia of being an excellent remedy for the effects of drinking. The influence of a continuous abuse of alcoholic drinks is first exerted

locally, but afterwards dyspepsia is produced; and the nutrition and functions of the entire economy, especially of the central portions of the nervous system, becoming interfered with, the blood itself being loaded with an injurious foreign material, the *dyscrasia potatorum* is at last completely established. The asarum fulfils various indications, acting beneficially on the alimentary canal in those cases in which the digestive powers are so much at fault. Its aromatic principle confers upon it a stomachic power, and regulates the condition of the intestinal discharges, producing vomiting and purging when given in large doses. Its most beneficial action, however, is manifested on the defective appetite, and by its counteracting the invincible longing for alcohol. The horrible sensations with which the drinker awakes in the morning, and which impels him to seek temporary and delusive relief from renewed libations, are much blunted and mitigated by means of a glass of strong infusion of asarum and some other nervine —*e.g.*, valerian. Its immediate effect is often to produce vomiting, and sometimes purging; but the painful sensations at the epigastrium undergo relief, and the appetite becomes invigorated. Persons who have been long habituated to alcoholic drinks cannot, however, have these suddenly suppressed with impunity; and in such cases the author gives the asarum in brandy, applying at the same time a blister or an issue to the pit of the stomach. By this means the normal activity of the stomach becomes excited and the longing for alcohol diminished. The author, however, cannot agree with those who would still allow a small quantity of spirits to habitual drinkers, even when the morbid desire for it has become appeased. The continuous use of a decoction of asarum, even when it does not succeed in extinguishing the desire for alcohol, always supports the powers of the patient; and it is remarkable in some cases, in which the individuals have been long accustomed to periodical intervals of drunkenness, ending in delirium tremens, how much longer these intervals will become, and how much less likely delirium tremens is to recur. The patients themselves are sometimes surprised at the comparative impunity with which they can continue their drinking. The author prescribes three or four glasses a-day of an infusion made with ℥iij. of asarum root, ℥j. of valerian root, and ℥½ of orange-peel, but he does not state the quantity of water employed. In cases of drunkenness another formula is composed of decoction of asarum (made by boiling from ℥½ to ℥j. of the root) ℥vj., tinct. of valerian ℥ij. to ℥iij., Sydenham's laudanum gtt. xij., syrup of orange-peel ℥½. A table spoonful of this is taken every two hours. He finds from two to five grains of bismuth taken four times a-day a valuable adjunct. He has also found the following popular Russian remedy of service in cases of drunkenness:—

℞. Ammon. carb., ℥½; aceti vini, lbj.; oxymel. scill., ℥½. Two tablespoonfuls every two hours.—*Med. Times and Gazette, July* 28, 1860, *p.* 91.

19.—ON THE THEORY OF SIMPLE EPILEPSY.

By Dr. CHARLES BLAND RADCLIFFE, Physician to the Westminster Hospital.

In the epileptic fit itself, the facts, when fairly read, admit of one conclusion, and one only. At the instant of the fall, a corpse-like pallor overspreads the countenance and the pulse dies out at the wrist—phenomena which seem to be only intelligible on the supposition that the arteries are nearly empty of red blood. A moment or two later, and the black and bloated face, the choking sounds, and the absolute suspension of all respiratory movements, show very plainly that the formation of red blood is arrested for the time. During the whole course of the convulsion, indeed, the state is one of suffocation. During the convulsion, that is to say, the supply of arterial blood is cut off at the fountain-head.

There is, however, one fact which, at first glance, might seem to show that there is an increased injection of arterial blood during the convulsion. Such injection is manifestly very imperfect at the onset of the fit, for upon no other supposition can we explain the corpse-like paleness of the countenance and the feeble and imperceptible pulse. But if the finger be kept upon the wrist during the convulsion, it will be found that the pulse will go on rising until it has acquired a force and fulness which it never had in the interparoxysmal period; and if the hand be placed on the breast, it will be found also that this rising of the pulse is accompanied by increased action of the heart. These facts are evident and unmistakable, but they do not show, as without reflection they might seem to do, and as they are often supposed to do, that *red blood* is being injected in greater quantity into the arteries during the convulsion.

When the process of respiration is arrested, the right side of the heart and the venous system generally are soon gorged and distended with black blood. Under these circumstances, indeed, the gorged and distended state of the right side of the heart may reach a point in which the folds of the tricuspid valve are forced widely apart, and an opening left through which the beatings of the ventricle are made to tell almost as much in driving the blood back through the auricle into the veins, as in sending it onward through the pulmonary artery into the lungs. But it is not right to suppose that the left side of the heart and the arterial trunks are empty of blood; and this may be readily verified by watching the changes which take place in the carotid and jugulars of a rabbit during the process of suffocation. On exposing the vessels, the artery is seen to be filled with red and the veins with black blood. On suffocating the animal by tying a ligature around its windpipe, the colour of the blood in the artery darkens rapidly, and in about two minutes and a half it is every whit as black as that of the blood moving in the neighbouring vein. Nor is the vein gorged and distended and the artery comparatively empty. On

the contrary, the artery is felt to pulsate as strongly under the rush of black blood as it did previously under the rush of red blood. Nay, *the pulse of the black blood is actually stronger than the pulse of the red blood;* for, on testing with the hæmadynometer, the late Professor John Reid (who first directed attention to these facts, and who has investigated the condition of the circulation in asphyxia more carefully than any other observer) found the mercury highest at the moment when the blood in the artery had become thoroughly venous and black.

A full pulse and a throbbing heart, therefore, must be looked upon as natural accompaniments of asphyxia; and thus the full pulse and the throbbing heart of the epileptic paroxysm, instead of showing that a larger quantity of *red blood* is being injected into the arteries at the time, may show that these vessels are then labouring under a load of *black blood*, as they do in asphyxia. And that the full pulse and the throbbing heart of the epileptic paroxysm must have this latter significance is evident, for the livid, black, and bloated head and neck, and the complete suspension of all respiratory changes, show very clearly that black and not red blood is coursing through the vessels at this time.

When the convulsion is over there is little to notice in the state of the circulation and respiration. When the spasms cease, the respiration is speedily re-established, and the re-admission of arterial blood into the system may be attended with some transient and inconsiderable febrile reaction; but this reaction has nothing to do with the convulsion, for when reaction is present convulsion is absent, and if convulsion returns, it is not till every trace of reaction has first taken its departure.

Regarding, therefore, these facts—the corpse-like paleness and the comparative pulselessness at the onset of the paroxysm, and the signs of positive and unequivocal suffocation by which this stage of paleness and pulselessness is succeeded—and remembering the previous arguments, which show that the convulsion is not to be ascribed to the presence of the stimulus of venous blood, there appears to be only one conclusion, and this is, that the convulsion of epilepsy is connected with the want of a due supply of arterial blood in the vessels.

Nor is it an objection to this view that the convulsions cease when the blood has become thoroughly deprived of its arterial properties. In order to discharge their office of conductors, it is certain that the nerves must be supplied with a sufficient quantity of arterial blood. If, for example, the principal vessel of a limb be tied, the nerves of that limb, wanting their due supply of blood, are unable to carry messages to the mind, or to transmit mandates from the mind to the muscles, until the collateral circulation is sufficiently established; and hence it is a fair inference that there must be a point in the process of suffocation where, wanting a due supply of arterial blood, the nerves must cease to be conductors, and where, consequently, the convulsions

will come to an end; for, upon any hypothesis, the convulsions will come to an end when the nervous centres cease to be in proper connexion with the muscles.

But, it may be asked, is there no change in the blood itself? Is there not some important truth in the "humoral theory of epilepsy," as recently advanced in this place by the late lamented Dr. Todd? "I hold," said this distinguished physician, "that the peculiar features of an epileptic seizure are due to the gradual accumulation of a morbid material in the blood, until it reaches such an amount that it operates upon the brain, in, as it were, an explosive manner; in other words, the influence of this morbid matter, when in sufficient quantity, excites a highly polarized state of the brain, or of certain parts of it, and these discharge their nervous power upon certain other parts of the cerebro-spinal centre, in such a way as to give rise to the phenomena of a fit. A very analogous effect is that which results from the administration of strychnia, which is best seen in a cold-blooded animal, like the frog. You may administer the drug in very minute quantities for some time without producing any sensible effect; but when the poison has accumulated in the system up to a certain point, then the smallest increase of dose will immediately give rise to the peculiar convulsive phenomena. This is the humoral theory of epilepsy. It assumes that the essential derangement of health consists in the generation of a morbid matter, which infects the blood; and it supposes that this morbid matter has a special affinity for the brain, or for certain parts of it, as the strychnia, in the case just cited, exercises a special affinity for the spinal cord. The source of this morbid matter is probably in the nervous system, it may be in the brain itself. It may owe its origin to a disturbed nutrition—an imperfect secondary assimilation of that organ—and in its turn will create additional disturbance in the functions and nutrition of the brain." And again: "According to the humoral theory, the variety in the nature and severity of the fits depends on the quantity of the poisonous or morbid material, and on the part of the brain which it chiefly or primarily affects. If it affect primarily the hemispheres, and spend itself, as it were, on them alone, you have only the epileptic vertigo. If it affect primarily the region of the quadrigeminal bodies, or if the affection of the hemispheres extend to that region, then you have the epileptic fit fully developed."

This theory is based upon the well-known connexion between the presence of urea in the blood, or carbonate of ammonia resulting from the decomposition of urea—the result of defective renal action—and one form of epileptiform convulsion; and it might also have been based upon the connexion between convulsion and blood overloaded with bile. But if there is any evidence in these facts in favour of the existence of this hypothetical morbid material, there is none in favour of the idea that the *modus operandi* of the material is in exciting a highly polarized state of the brain, if by this state is meant anything

like a condition of excitement. On the contrary, it is certain (as will be shown in the next lecture) that the action of the brain and of the nervous system generally is reduced to the very lowest ebb at the time when convulsion is brought about by the accumulation of urea and bile in the blood; and it is not less certain that strychnia, instead of acting as Dr. Todd supposes it to act—that is, by exciting a highly polarized state of certain parts of the nervous centres, acts by reducing the stimulating powers of the blood, and by diminishing the electrical action of both nerve and muscle.

There is little doubt, however, that retained excretions must play an important part in the production of epilepsy. A free discharge in the office of excretion, not only in the kidneys and liver, but in every excretory organ, is essential to the preservation of healthy blood; and it may well be believed that an imperfect discharge of the office of excretion, *in one or other of the excretory organs*, may lead to the accumulation of effete matter in the blood, and that this accumulation of effete matter may be a not unimportant cause in bringing about an attack of epilepsy. But there is no reason for supposing that the blood under these circumstances becomes more stimulating. On the contrary, the conclusion which arises out of the history of the cases where the urine or bile is suppressed, is the natural conclusion, and this is, that blood thus altered is less fit to discharge its several offices; in other words, less stimulating.

Nor does there appear to be any reason for supposing that venous congestion has a more important part to play in the production of epilepsy than that which has been assigned to arterial injection. No doubt the veins of the brain and head generally are congested from a very early moment, but there is a moment antecedent to this in which the death-like pallor of the face is a sufficient proof that the veins were emptier than usual before they became congested. At any rate, the acknowledged anatomical difficulty must be overcome before it can be supposed that Dr. Marshall Hall's hypothesis of *trachelismus*—or the prevention of the return of blood from the brain by the spasm of certain muscles in the neck—has anything to do with the causation of epilepsy.

It would seem, then, as if there was something utterly uncongenial between epilepsy and arterial excitement. It would seem, indeed, as if the spasms, as well as the loss of consciousness and sensibility, were connected with want of arterial blood—empty vessels in the first instance, vessels filled with black blood afterwards. It is not improbable, also, that the blood may have been previously rendered less stimulating by the retention of something which ought to have been eliminated by one or other of the organs of excretion. In a word, the phenomena are entirely in harmony with the previous considerations respecting muscular motion; for according to them, the action of arterial blood is to antagonize contraction, and not to cause it.

Interrogating the nervous system, the facts are found to have that theoretical significance which the state of the circulation and respiration would lead us to expect.

These facts will scarcely warrant the idea that epilepsy is connected with anything approaching to over-action of the *brain proper*. On the contrary, everything seems to point to a state which is the very opposite of over-activity. Thus, the comparative want of memory, intelligence, fancy, and purpose, which marks the interparoxysmal condition; the utter annihilation of everything mental in the fit itself; and the gloom and prostration following the fit, are facts which can have no double meaning.

Nor is a contrary opinion to be drawn from the morbid appearances which are disclosed after death. If these chance to indicate previous inflammation, it does not follow that convulsion had any direct connexion with the inflammation as inflammation; on the contrary, the convulsion may have happened before or after the inflammation, when the energies of the brain were prostrate or exhausted—an alternative which we shall see to be the correct one when we come to speak of epileptiform disease connected with special disease of the brain. And surely it is not possible to draw any but one conclusion from the appearances which are common to epilepsy and dementia—pallor of the grey substance, atrophy, chronic softening and induration, dropsical effusion, and the rest?

But what of the state of the *medulla oblongata?* for, as Professor Schrœder Van der Kolk has well shown, the seat of the characteristic spasms, the bilateral character of the spasms, and the appearances presented after death, all point to this organ as one which is specially concerned in bringing about the epileptic paroxysm.

The spasms of epilepsy begin in muscles which receive nerves from the medulla oblongata—in muscles, that is to say, which are supplied by the facial, the accessory, the hypoglossal, and the portio minor trigemini; in slighter cases they are limited to these muscles. The spasms of the walls of the chest and abdomen, which are the most prominent and marked features in the complete attack of epilepsy, and which may be so fierce and unyielding as to cause fatal suffocation, also point to the same nervous centre; for a similar state of things is brought about by the action of a strong stimulus upon the great afferent nerve of this centre—the pneumogastric.

The bilateral character of the spasms is another argument that the medulla oblongata is especially affected in epilepsy. The lateral halves of this organ are connected in the most intimate manner by transverse fibres and commissures—much more intimately than the lateral halves of the brain and spinal cord; and hence it is that the corresponding nerves belonging to the two sides of the medulla oblongata are under a stronger physical necessity to act together than that which rules the corresponding nerves belonging to the two sides of the brain and spinal cord. In the case of the two latter centres, the

nerves belonging to one side may be paralysed or otherwise affected, without any obvious injury to the nerves of the other side; but not so in the case of the latter centre. Indeed, it is evident that the actions which emanate from the latter centre—the play of the features, the motion of the tongue, the vocal adjustments of the larynx, the respiratory movements, &c.—must at once come to an end unless there be the strictest sympathy and concert in the action of the corresponding nerves of the two sides. Now, in epilepsy the spasms are always more or less *bilateral*, and for this reason, therefore it may be supposed that they have some special connexion with a nervous centre of which one lateral half cannot act without the other.

The appearances after death point also to the medulla oblongata as especially concerned in the production of epilepsy. In an early stage of the disorder, we may fail to find any characteristic changes; but, in confirmed cases, the texture is harder than natural, from the interstitial deposit of a minutely-granular albuminous matter, or else softened, swollen, and exhibiting signs of evident fatty degeneration. The posterior half of the medulla oblongata is redder and more hyperæmic than it ought to be; and, on examining the bloodvessels in this congested portion, they are found to be of thrice their natural dimensions, and with their walls much thickened and altered—this dilatation and alteration being chiefly in the corpus olivare and in the course of the hypoglossus in the case of epileptics who bite their tongue, and in the course of the roots of the vagus in the case of epileptics who do not bite their tongue.

It is evident, then, that the medulla oblongata is especially affected in epilepsy; but it does not follow, as Professor Van der Kolk supposes, that the essential cause of the convulsive affection is to be found in an exalted sensibility and activity of the ganglionic cells of this centre.

In favour of this view,—that epilepsy is dependent upon exalted sensibility and activity of the ganglionic cells,—appeal has been made to the fact of spasm, to the presence of a full, bounding pulse, and to the freedom from attack which is for some time the fruit of an attack, particularly if this has been violent; but the answer is not necessarily that which Professor Van der Kolk supposes it to be. After what has been said about muscular motion in the first lecture, it is not possible to allow that spasm in itself is an argument in favour of exalted sensibility and activity in ganglionic cells. After what has just been said about the phenomena of the circulation in epilepsy, it is impossible to allow that the condition of the circulation favours spasm by bringing about a more active state of the medulla oblongata, (the functional activity of this, as of every other organ, being in direct proportion to the activity of the circulation of *red* blood in the organ;) for it has been seen that the bounding pulse, to which reference is made, is filled with *black* blood, and not with red. Nor can the freedom from attack, which is for some time the fruit of an attack, be

appealed to as a certain proof that the attack is the sign of the discharge of some overcharge of excitability previously present. On the contrary, it may be argued with some degree of plausibility, from certain facts which have to be mentioned in the next lecture, that the attack was preceded by depression of the circulation and innervation, that the convulsion supervened when this depression had reached a certain point, and that the recurrence of the attack was prevented for a time by the state of reaction in the circulation and innervation, which is a consequence of the convulsion. The case may be one, indeed, of which the history of the rigors of ague may serve as no inapt illustration; for here we have, first, the circulation failing more and more until the bathos of the cold stage is reached; and. secondly, a state of reaction which banishes the rigors most effectually so long as it continues.

It would even seem as if appeal might be made to the appearances after death, and to the actual condition of the circulation in the fit, for positive arguments against the idea of anything approaching to exalted action of the medulla oblongata.

The signs of fatty degeneration can have but one significance—under-action, not over-action. The interstitial deposit, also, implies an equivalent absence of healthy nerve-structure, and so does the dilated condition of the bloodvessels; and this absence of nerve-structure must necessitate a corresponding absence of nervous action. The appearances after death, indeed, if they show anything, show that the medulla oblongata of the epileptic is *damaged in structure*, and because damaged in structure, *weaker in action*, than it ought to be.

The great argument against the idea of anything like over-action of the medulla oblongata in epilepsy, however, is to be found in the state of the circulation; for if, as may safely be assumed, the activity of any organ is in direct relation to the activity of the circulation of *red blood* in that organ, how far from anything like over-action must be the state of things in which, as is the case in the epileptic paroxysm, the vessels are at first comparatively empty of red, and afterwards completely filled with black, blood?

Nor can the curious discovery of Dr. Brown-Séquard, that certain injuries of the spinal cord are followed by an epileptiform affection, be construed into an argument that there is anything like a state of exalted action of the *spinal cord* in epilepsy. This curious result, which is brought about by puncturing or dividing more or less completely almost any part of the spinal cord, is developed, not immediately, but in the course of three or four weeks after the injury. The attacks, once developed, occur spontaneously at various intervals, often several times a day; they may also be brought on by pinching or otherwise irritating the portion of the skin which corresponds to the region of the whiskers in man. This excitable spot is supplied by twigs belonging to the sub-orbitary, the auriculo-temporalis, the second, and perhaps the third, cervical nerves; and it is a curious fact, that

the irritation which brings on a fit when applied to the skin in which these twigs terminate has no such effect when applied to the twigs themselves. Any other part of the skin may be pinched or irritated with impunity, but this one spot can scarcely be touched without at once bringing on a fit.

These facts are very curious, and, in the main, very unintelligible; but this much at least is evident, that they do not countenance the idea of any over-action of the spinal cord in epilepsy. The fact that the epileptiform affection does not make its appearance until four or five weeks after the injury would appear to show very clearly that the fits have nothing to do with that local inflammation in the cord which may be supposed to have been set up in the first instance by the injury. After such a lapse of time, indeed, is it not the natural conclusion, that any over-action of the cord arising from the inflammation produced by the injury must have died out, and left the cord damaged, weakened, under-acting? Nor is a contrary conclusion to be drawn from the excitable condition of the nerves proceeding from the neighbourhood of the cheek or cheeks; for both sides of the face are thus affected, if both sides of the spinal cord have been injured. What the full significance of this curious fact may be we may have yet to learn, but at any rate there is no reason to suppose that this excitable condition of the skin implies an over-acting condition of the nerves or nervous centres concerned in the phenomenon. The excitable portion of skin is not over-sensitive, for the animal manifests no signs of uneasiness when it is handled immediately after a fit. Over-sensitiveness, moreover, would seem to have nothing to do with the matter. At any rate, pain, and not convulsion, is the consequence of handling those portions of the skin of the animal which may have been rendered highly hyperæsthetic by the injury to the cord which brought on the convulsions. It is certain, also, that a somewhat similar condition of excitability is brought on when, as in several experiments related in the first lecture, the skin is cut off from the full influence of the nervous centres; and hence the natural inference would be, that the action of the nervous centres in the epileptic guinea-pig is *minus* rather than *plus*.

As in the former instances, however, so here, we turn to the condition of the circulation and respiration in order to know what is the actual functional condition of the spinal cord in epilepsy; and so turning, we see that the action of the cord under these circumstances must be almost or altogether *nil*. For what action can there be when little or no arterial blood is injected into the vessels?

A similar argument will also dispose of the idea of over-activity of the *ganglia of the sympathetic system* as a cause of epilepsy. It is very possible that the contracted state of the arteries, which is implied by the death-like pallor of the countenance and the comparative pulselessness at the wrist, may show that the coats of the vessels are in a state of spasm; and it is also possible that the cause of this

spasm may have to be sought in the sympathetic system; but it does not follow that over-action of this system is this cause. On the contrary, the experiments of Drs. Kussmaul and Tenner show most conclusively that strong epileptiform convulsion is possible when the action of the sympathetic ganglia is entirely suspended by arresting the supply of blood to these organs.

And certainly no opposite conclusion is to be drawn from the vague and undefinable sensations or movements, very varying in character, but all comprehended under the term *aura*—sensations of pain, numbness, tingling, or a feeling of 'cold vapour; movements of shuddering or spasm, beginning in a distant part and travelling towards the head; for the most probable interpretation of these symptoms is that of Dr. Watson—that they are in some degree analogous to the numb and tingling feelings which are the frequent precursors of paralysis and apoplexy, or to the globus of hysteria—phenomena which by the most perverse process of reasoning can scarcely be supposed to indicate other than a state of defective innervation somewhere.

But, it may be asked, is there nothing else? Is there no peculiar state of the nervous system in epilepsy? Is there no *morbid irritability?* In order to answer this question, it is necessary to ask another—What is morbid irritability? It is not inflammation; it is not fever; it is some indefinable and negative state which occurs frequently in teething, in worm disease, in uterine derangement, and in many other cases—a state in which the patient is unusually depressed by depressing influences, and unusually excited by exciting influences. But what is this state? Is it anything more than mere exhaustion? In difficult teething, the strength is worn away by pain and want of sleep; in worm disease, the parasites help to starve and exhaust the system; in uterine derangement, the health is undermined, in all probability, by pain and by sanguineous or other discharges. In each case there is unequivocal exhaustion of body and mind, and the signs of morbid irritability appear to be nothing more than the signs of such exhaustion. A weak person is more affected by the several agencies which act upon the body from within and from without, and he is so because he is without some of that innate strength which belongs to the strong person; and the person who is morbidly irritable is, in reality, one who, for want of this principle of strength, responds impatiently to the several stimuli, whose office it is to elicit his vital phenomena. In a word, this undue morbid irritability may be nothing else than the natural consequence of that general want of power, the signs of which are written so legibly upon the vascular and nervous systems of the epileptic. There is no necessity, then, to look upon this morbid state of irritability as an evidence of the existence of any peculiar condition in some part of the nervous system; for, thus interpreted, it only shows that the state of muscular contraction is ill antagonized by nervous influence. Thus interpreted, indeed, morbid irritability only becomes another name for inefficient innervation.

The *theory of simple epilepsy*, therefore, which may be deduced from a consideration of the facts relating to the nervous system is in harmony with that to which we have been led by a review of the state of the circulation and respiration in the epileptic; and this theory is one which tallies as completely with the view of muscular motion set forth in the first lecture, as it disagrees with that commonly received opinion according to which the muscles are supposed to contract convulsively because they are subjected to excessive stimulation.—*Lancet, May* 26, 1860, *p.* 512.

20.—*On the Use of Faraday's Electricity in Chorea.*—In this disease, the action of this therapeutic agent differs widely, according as it is applied to the skin or transmitted through the muscles. Faradization of the muscles checks the irregular contractions, during the whole time of the passage of the current; but when this is intermitted chorea reappears with the same violence as before the operation, and no trace whatever remains of the electric action. Muscular faradization is therefore inefficacious for the cure of chorea; but it may be advantageously resorted to to contend with asphyxia, one of the most dangerous symptoms of the disease. To effect this purpose, the current must alternately be transmitted through the inspirator and the expirator muscles. Faradization of the skin, on the contrary, is applicable to all cases of chorea, and occasions a very rapid and marked diminution of the movements, and frequently effects a prompt cure of the malady. M. Briquet applies Faraday's electricity to the skin every day or every other day, during five or six minutes, along the entire length of the affected limbs, and chiefly upon the parts most convulsed. In eight choreic girls so treated, the complete cessation of the irregular muscular action was obtained once in eight, in a second in twenty-one days, and in the others in twenty-four, twenty-eight, thirty-three, thirty-six and forty-seven days. One patient left the hospital after a fortnight imperfectly cured. In the majority of the cases the usual methods of treatment had been unavailingly resorted to for a space of from six to eight and twelve weeks; the faradization of the skin may therefore be asserted evidently to hasten the the cure of chorea.—*Dublin Hospital Gazette, May* 15, 1860, *p.* 158.

21.—*Is the Brain in a state of Congestion or not during Sleep?* By JOHN HILTON, Esq., F.R.S., Surgeon to Guy's Hospital.—[The general opinion is that during sleep, which is a kind of slight coma, the brain is in a state of congestion, that the pia mater and other plexuses are more or less pressing slightly on the brain. Mr. Hilton brings forward some experiments by Mr. Durham, one of the Demonstrators at Guy's Hospital, to prove that this opinion is at the least very doubtful.]

A dog having been chloroformed, a portion of the bone, about as large as a sixpence, was removed from the parietal region of the skull by means of the trephine; the subjacent dura mater was cut away, and the surface of the brain exposed. As long as the animal continued under the influence of chloroform, the smaller vessels of the pia mater were turgid with dark coloured blood, and the larger veins were considerably distended. No difference in colour between the arteries and veins could be recognised. The exposed portion of the brain manifested a tendency to rise into the opening through the skull. By and bye the immediate effects of the chloroform passed off, and the animal sank into a comparatively natural and healthy sleep. A very marked change in the appearance of the brain accompanied this change in the state of the animal. As sleep supervened, the vessels gradually emptied themselves; the veins ceased to attract notice by their distension; the exposed surface of the brain sank down to, or below, the level of the opening, and became pale in colour. In the course of a short time the animal was roused, and irritated. A blush seemed to start on the surface of the brain; the vessels of the pia mater became fuller and fuller, and of a bright arterial hue. The contrast between the appearance of the brain during this state of functional excitement and the previous state of quiescence was most striking. The more the animal was excited, the fuller of blood did its brain appear to become, and the higher did the exposed portion rise above the general level. When the animal was allowed to return to its state of repose, the brain again sank down, and reassumed its pale aspect. A full account of these and other confirmatory experiments and observations will be read at the British Association meeting at Oxford.—*Lancet, Sept.* 1, 1860, *p.* 206.

DISEASES OF THE ORGANS OF CIRCULATION.

22.—ON THE SIGNIFICATION OF THE HEART'S THROB.

By J. Maclise, Esq., F.R.C.S.

In daring, now-a-days, to state my conviction that the throb of the heart, as felt in the left pectoral region of man or animals, is due, not to the systolic action or contraction of that organ, but really to its diastolic motion or expansion, it may well be said that I should be furnished with good reasons for such belief, since the enunciation of it is in opposition to all recorded physiological opinion and the consequences thereby incurred.

When we reflect, however, that the opinions of even the highest authorities on a question of this nature cannot be otherwise, under the anatomical circumstances, then more or less speculative according as the veiled phenomena happen to be interpreted, this should give a place in the arena of debate to any one disposed to enter. The heart,

as functional within the enclosing thorax, is impossible to be demonstrated. Its motions of systole and diastole, which we infer, (and reasonably) to be necessary for circulation, are performed out of view. And when, after experimental mutilation of form and mechanism, the heart is exposed, we see, instead of the free expansion and contraction which we expected to witness of such an organ, but a mere alternating spasm and relaxation of its muscular fibres without producing any change in the capacity of its interior. Whether the heart be detached from its connexions and removed from the thorax, or still holding its natural relative position by its vascular trunks, it manifests characters in common with all muscular substance. In neither condition can we discover the organ performing that motion, which would account for its throb—its measured, positive, dynamic impulse anteriorly against the left thoracic side; and, because there are anatomical facts to prove that the heart's systolic action cannot effect this beat, and hence leave us no choice but to regard it as the effect of diastole induced by some physical condition of the organ in the thorax of the living body, and only when so situated, I have no hesitation in laying before your readers the two following propositions:—

1. *The heart's throb cannot be caused by its systolic action.*
2. *The heart's throb can be caused by its diastolic motion.*

Amongst the anatomical evidences which appear to me to prove the first proposition directly, and the second indirectly, I need only mention a few, for these are sufficient:—The living heart *in situ* is enveloped by the lungs in all parts of its periphery with the exception of a portion of the right (anterior) side of the right ventricle, near its base, which portion, of greater or less area in different cases, is placed close behind the fourth, fifth, and sixth left costo-sternal cartilages. The apex of the heart is at all times, both during inspiration and expiration, covered by the left lung intervening between it and the thoracic wall. The apex, therefore, cannot strike the thoracic left side, as it is supposed to do by those who believe that the heart's throb is owing to systolic action; and, even if such a locomotion of the heart were otherwise possible, the closely investing bag of the fibro-serous pericardium, which binds the organ to the diaphragm, would be sufficient to prevent its occurrence. If, then, to any particular part of the heart, by reason of its being in close apposition with the thoracic parietes where the throb is most appreciable, that impulse can justly be assigned, it can only be that portion of the body of the right ventricle which is permanently uncovered by the left lung; and, in respect to this part, it remains to be considered whether the throb is caused by its diastolic or its systolic motion.

The physiological phenomena which, I think, should induce us to ascribe the heart's throb to diastole of the body of the right ventricle, and to this part only, must of course negative the assumption that it can be the effect of systole, or of any other of those many various and oftentimes futile causes which have been recorded.

The heart, as a hollow muscular organ, must perform its contractions, although these be vital, according to the physical law—that is to say, the larger space which the heart, while diastolically full, occupied in the thorax, must in respect to that organ be diminished when this is systolically expelling its contents from its cavities through the arterial system. The diminution of the heart by systolic contraction on the blood is answered by the dilatation of the artery as recipient of that fluid; and, conversely, the diastolic enlargement of the heart as recipient of blood must be answered by recession of the artery as conductive of the blood. This being the, so to speak, natural librative motion of the organ and its arterial vessels, it ought to appear, I think, at first sight self-evident that of the two states—viz., increase of volume or capacity, and decrease of the same—the former is the more likely to cause the pectoral impulse or throb, and as if signifying that the diastolic heart knocked at the breast for larger room, so as the more amply to receive and pass the rising venous tide.

When the heart has performed systole, all its diameters have become lessened towards a common centre, which is in its septum; and consequently it may be reasonably assumed that the external paries of the right ventricle must tend rather to separate itself from the thoracic wall than be impelled against it, for now the blood is in efflux from the organ. On the contrary, when the heart is being set in diastole, all its diameters become increased from the same cardiac centre; and, necessarily, the wall of the right ventricle must become suddenly pressed against the front of the left thoracic wall, for now the blood is in afflux to the heart; and that its entry into the organ is *sudden*, I, for my own part, find every reason to believe. Systole, or action, is followed by diastole as quick, as reaction; and these being the only two motions of which the heart is capable, and in the performance of which, alternately, without interval, through life it is engaged, the latter motion, I am persuaded, for the reasons specified, is the cause of the pectoral beat. I have found it possible to imitate it in the dead subject in this manner:—Having tied the brachio-cephalic veins as closely as possible to their origin in the superior vena cava, the free-vented nozzle of a large syringe full of size solution was inserted into the inferior vena cava close to the foramen quadratum in the diaphragm. The thorax was left, as naturally, entire; and the lungs were fully dilated through the trachea, so as to support the heart laterally and posteriorly, and keep the right ventricle in apposition with the left costo-sternal region. On placing my hand over this region, while the syringe by a quick motion of the piston-rod suddenly injected that ventricle, I felt an impulse similar to the heart's throb. Again, in order to prove that it is to the diastolic part of this ventricle which is uncovered by the lung, and not to the systolic apex of the heart, that the throb is due, the heart, circumstanced as above, was injected with wax, and this allowed time to set, when, on exposing the organ, the body of its right ventricle was found to have been flat-

tened against the corresponding part of the thorax. This flattening I take to be indicative of the natural tendency of the diastolic motion being sternal, and therefore that it is this motion which causes the throb. With this conclusion all the other vital phenomena accord. The heart's beat and the arterial pulse are not synchronous, which necessarily they would be if the former were the effect of systole.—*Lancet, July* 28, 1860, *p.* 95.

23.—ON THE PREVALENCE OF HEAT APOPLEXY AMONG SOLDIERS DURING THE HOT WEATHER CAMPAIGN IN 1858.

By Dr. CHARLES ALEXANDER GORDON, C.B., Surgeon, 10th Regiment.

[Generally speaking, it is not among the soldiers who are permitted to go out freely during all seasons and in the heat of the day, that sun-stroke is most liable to occur. Confinement of the men to their barracks during a great part of the day in the hot season, is a fertile source of tubercular disease, yet this practice is extremely common in India.]

It is the subject of common remark in India, that one of the meteorological conditions under which "heat apoplexy" chiefly occurs, is when the breeze for a time ceases,—the sky becomes obscured by a film of dark, negatively electrified clouds, and a sense of oppression hangs like a weight upon the mental as well as bodily energies. At such times, not only do men become the subjects of this disease, but the lower animals are not unfrequently attacked, and die suddenly from it.

During the operations against the rebels in the Arrah district, a very remarkable influx of cases of this disease occurred among the British portion of our force on 23rd and 24th May, 1858; and from letters subsequently received, we learned that a very remarkable number of seizures occurred on the same dates among the soldiers employed on field service throughout the Presidency.

This circumstance tends to confirm the opinion, that the proximate cause of the disease is lost balance of the cerebro-spinal and sympathetic nervous power, induced, no doubt, by particular modifications in the conditions of atmospherical influences, the precise nature of which our present means of investigation do not enable us to detect.

Recent occurrences in India have attracted attention to deaths from what is called sun-stroke more than heretofore; the experience of medical officers in that country impressing them with the opinion, that soldiers, if not exhausted by long previous service on a campaign, run comparatively little risk of attacks of this nature, however great

may be their exposure, so long as they are successful in the military operations in which they are employed.

It is far otherwise, however, when the system is lowered by previous severe duty, insufficient food, by disaster, or any other of the ordinary causes that depress the vital energies: under such circumstances the men are prone to become affected by the disease when exposed to the sun, more especially if they have to march in "close order" through jungle tracts, where there is very imperfect circulation of air.

Mr. Hill publishes a tabular summary of all the recorded cases of the disease to which he has access. The table contains records of 47, and of this number the following are the alleged causes of the attack:—

Exhaustion, fatigue, and exposure to the sun	5
Exposure to sun alone	9
Cause not noted	2
Heat in camp	6
Drink and exposure to sun or hot winds	9
Drink, exhaustion, and heat (whether with exposure not stated)	12
Heat, without exposure and without drink	2
Heat alone (not stated whether or not with exposure)	2
Total	47

—*Indian Annals, July*, 1858.

We thus find that exhaustion or fatigue has had more or less to do with the disease in seventeen of all the cases above noted.

Next to depression from long-continued fatigue or imperfect food, the most prevalent predisposing cause of heat apoplexy is the habit of dram-drinking, in which all soldiers indulge more or less. During the hot season of 1855, it was found that men of the Bengal Artillery, stationed with the 10th Regiment, died in great numbers from sun-stroke, while our soldiers did not suffer in an unusual proportion from the disease. Inquiry elicited the fact that the men of the Artillery, each day at noon, ran through the intense heat from their barracks to the canteen, rapidly swallowed their allowance of rum, ran back, partook of a heavy meat dinner, and threw themselves upon their cots, where they snoozed away, in half-unconsciousness, till the time arrived for them to dress for their evening exercise. The men of the 10th Regiment, at the same station, were never permitted to partake of spirits until after sunset. This plan was subsequently adopted in the Artillery, and the disease ceased.

In *Indian Annals* of July, 1858, it is stated that when the 67th Regiment was ordered from St. Nicholas Mole, St. Domingo, upon an expedition up country, the troops, previously to marching off, were supplied with a full ration of spirits. It was, as might have been foreseen, speedily consumed; and the men marching through a dry, sultry country, that furnished no water, fell down at almost every step. Nineteen actually died upon the road.

The system of allowing the soldiers to dine in the middle of the day, while there is a prevalent tendency to heat apoplexy, is objected to by many medical officers. While we were employed on field service during the hot season of 1858, the soldiers were permitted to dine, when practicable, and they wished to do so, in the evening. The experiment was upon too small a scale to justify any conclusions being drawn from it, nor did any particular result follow. If we are to judge from what occurred in the 20th Regiment at Lucknow, however, the dangers of the soldiers of taking heavy mid-day meals at such times is apparent, and seems among these men to have induced this disease.

Dr. Cheans states, that "On 10th June, (1858), the 20th Regiment marched out of Lucknow, and reached Allumbah about 9 a.m. From want of tents, the men were exposed to the sun's rays and heat of the sun for several hours.

"Before evening, 56 admissions had taken place in a force of 497. Three men died within four hours. Next day, 23 admissions from fever; on 12th, 5 admissions, 4 of which showed symptoms of apoplexy, 1 proving fatal about 3 a.m.; the fifth was a case of cholera.

"During the remaining three days there occurred 6 cases of heat apoplexy, 1 of which proved fatal; 2 of cholera; and 70 of fever. It was particularly observed that most of the cases of heat apoplexy occurred between the hours of 3 and 7 p.m., when the men had dined, and the sun falling, the heat had become more sensible and oppressive."

It is certainly not a little remarkable that, notwithstanding the severe duty and exposure to which our men were subjected at different times between 1st April, 1857, and 31st March, 1858, not one became affected with heat apoplexy; dysentery having been the disease by which almost the entire mortality was occasioned. Vertigo and other premonitory symptoms occurred by no means unfrequently; but they did not attain that degree of intensity that would constitute pure sun-stroke,—cold affusion and brief seclusion from direct solar heat being all that was necessary for their recovery.

Other regiments, however, were less fortunate; and personal knowledge enables me to say that soldiers were most liable to the disease when, after a debauch of spirits over night, they were forced to march armed and accoutred during the heat of the succeeding day. This was only what might have been anticipated.

In an inquiry into the proximate cause of heat apoplexy, we shall find the remarks of Mr. Hill to be well worthy of our consideration.

This author alludes to one of the well-known effects of exposure to great heat, by which the oxygenating power of the atmosphere becomes impaired as a consequence of the expansion created by increased temperature. He then goes on to make some observations, the truth of which was illustrated in my experience of the disease during the mutinies.

It follows, that the oxygenating power of the atmosphere being diminished, the respiration must become accelerated; the circulation also increases in rapidity, and, as a result, the temperature of the body rises. The imperfect decarbonization of the blood which results from this condition, is in a measure illustrated in everyday life in India; giving rise, as it no doubt does, to the listlessness, lassitude, want of physical energy and mental vigour, as also to the extraordinary tendency to somnolence, which manifests itself in persons newly arrived in that country.

As the expansibility of the circumambient air increases, so likewise does its capability of receiving other gases, whether in the shape of malaria, miasma, or effluvia, emanating from the earth or disengaged from soils or other sources, often under circumstances inexplicable and conditions unknown. Moreover, this expanded condition of air favours the more ready disengagement of these noxious principles. It follows, that a great quantity of pernicious ingredients are inhaled, and are in an unusually concentrated form brought in frequent and close proximity to the blood, the absorbing powers of which are increased by the circumstances which give rise to the conditions now under notice.

The skin is an outlet for pernicious products that are taken into the system. If it perform its functions in a proper manner, they will be quickly expelled; but if this conservative function ceases by perspiration being checked, or from any other cause, the inhalation of these noxious principles continuing,—then, indeed, are induced a succession of evils constituting in their integrity an attack of fever or of heat apoplexy, according to the other causes which may be in operation at the time.

Diminished evaporation from the skin in a most hot condition of atmosphere thus explains why we then suffer most (in health), and why hot climates are most unwholesome. A moist state of atmosphere is also favourable to absorption: hence night is the most unhealthy time in malarious districts, because then morbific emanations are most readily taken into the system; and hence also, no doubt, arises the circumstance, already alluded to, that attacks of the present disease more frequently occur in persons during the night-time than when they are exposed to the direct rays of the most powerful sun.

Another circumstance of no little importance may also be explained on the same supposition—namely, the greater liability of men to be attacked by the disease when they are crowded in masses than when they are in parties of inconsiderable size, or altogether isolated; as also the fact, that a body of soldiers, marching in close column along a narrow road through dense brushwood, where the atmosphere is but little agitated, are far more liable to suffer from heat apoplexy than they would be if marching along the open country, where the poisonous emanations arising from their own bodies are instantly carried

away by the breeze, even if that breeze be intensely hot, as it certainly is in India during May and June. Of this we had numerous illustrations during the campaign against the Sepoys; and I may here observe that our great preventative was abundance of water, carried by natives, by camels and elephants, with which soldiers and officers had continually to drench themselves while pursuing the Sepoys through the tracts of jungle, as, for instance, in the province of Bahar, where they took shelter.

Mr. Hill accounts for heat apoplexy thus:—

1. The lungs are the organs primarily deranged; becoming surcharged with blood, rendered impure by noxious products. The good effects of cold affusion may thus be briefly stated:—As stimulating the flagging respiratory functions through excitomotory influence, moistening the surface, by which exosmosis and endosmosis are established; and, from evaporation being induced, the temperature of the body is directly reduced.

2. Nature determines to the skin; and its functions, being already weakened or deranged, become ultimately stopped. It thus becomes dry: dry and highly congested as it is, superficial oxygenation of the blood partially goes on, and hence the increase of heat.

3. The brain is not primarily affected, but becomes so from the derangement of arterialization of the blood.

4. The congestion of blood in the vessels of the membranes (and skin) is apparently due to a loss of tonicity in the structures, and an inability in the heart to overcome this congestive state; and it may be noted, if this explanation be correct, that it may account for the benefit of arteriotomy, by taking the oppression directly off the heart, which venesection would not do.

Heretofore it has been found that the abstraction of blood in a case of sun-stroke was almost invariably followed by death,—the operation of leeches was too slow; in fact, medical treatment in its more severe forms was almost powerless, and hence the frightful rate of mortality already stated.

It is to be remarked, however, that the general rule was to abstract blood from a vein, usually at the ordinary place of venesection; and that with this view, the patient was made to sit up in bed, or was supported in that position.

In these cases it frequently happened that death was almost instantaneous; while, on the other hand, the fatal issue scarcely seemed to be accelerated if venesection was not employed, or, in fact, whatever was the remedy brought into use.

[Dr. Gordon then proceeds to relate two cases which were treated by freely throwing cold water over the head and upper part of the body; then a brisk purge was given of calomel and croton oil. In one case this sufficed; in the other, in which the symptoms were fully developed, the temporal artery was divided and allowed to bleed

freely, and vesication on the nape of the neck was occasioned by the application of a hot spatula. Dr. Gordon sums up the paper thus:]

The following, as a rule, are the measures from which advantage is to be chiefly expected in the treatment of heat apoplexy:—1. Arteriotomy; 2. Cold affusion to the head and epigastrium; 3. Rapid counter-irritation, by the application of a hot spatula to the nape; and 4. Internal counter-irritation, by the administration of croton oil.—*Edinburgh Med. Journal, May* 1860, *p.* 986.

24.—ON THE USE OF LARCH-BARK IN HEMORRHAGES.

By Dr. J. M. O'Ferrall, First Medical Adviser to St. Vincent's Hospital, Dublin.

There is no accident which occasions more anxiety to the physician, as well as to the patient, or his friends, than internal hemorrhage, and none that from its suddenness and danger renders empirical treatment more justifiable. In such a case time is everything; life is ebbing away, and we cannot wait for the development of the physiological effects of medicines, the principle of whose action we fancy we understand. There is no occasion, therefore, in which we require to be provided with so many expedients in the event of failure of those we have already tried. When a new styptic is proposed, it becomes the duty of the clinical physician to avail himself of the opportunities afforded by a large hospital, and to record as early as is consistent with accuracy the results of his experience. The larch-bark has been proposed as a remedy in purpura, and analogy naturally suggested it as a means of controlling similar conditions of the mucous membranes, often of an alarming nature. The first class of cases in which I directed its employment was the intercurrent hæmoptysis of phthisis; in some of these, the usual remedies—ipecacuanha, lead, tannic or gallic acids, &c., had been employed without effect, when the larch-bark was tried. The hemorrhage became diminished and soon ceased, and the remedy rapidly began to acquire an importance in our estimation. As confidence in the drug increased, it took an early place in the prescriptions for such cases, and up to the present moment has given such satisfaction, that it is the prevailing remedy in my wards for this affection.

There is another class of cases, often of an exceedingly obstinate nature, in which it has proved of signal service—cases of uterine hemorrhage. Some of those cases have depended upon the presence of fibrous tumours connected with the uterus; others upon carcinomatous degeneration of the organ. In both the use of larch-bark has been followed by a cessation of this dangerous accident. In some cases of hæmaturia from various sources—renal or vesical—the blood in the urine disappeared under its use.

In hæmatemesis I have not yet ventured to try this medicine, as I have not experienced failure from the ordinary remedies—turpentine, gallic acid, &c., since its introduction.

The form in which I am in the habit of exhibiting the larch-bark is that of the tincture, in doses of half-a-drachm or a drachm every third hour.—*Dublin Hospital Gazette, July* 2, 1860, *p.* 199.*

DISEASES OF THE ORGANS OF RESPIRATION.

25.—ON THE LARYNGOSCOPE.

(By a Berlin Correspondent of the 'Med. Times and Gazette.')

[The laryngoscope was an instrument formerly in use by the profession. It was originally invented by an English surgeon.]

In Liston's 'Practical Surgery,' page 417, we read, under the head of "Ulcerated Glottis," the following remarks:—"A view of the parts may be sometimes obtained by means of a speculum—such a glass as is used by dentists,—on a long stalk, previously dipped in hot water, introduced with its reflecting surface downwards, and carried well into the fauces." This pregnant hint of Liston's remained unnoticed till 1855, when Garcia published a most valuable series of auto-laryngoscopic investigations, instituted for the purpose of elucidating the mechanism of the human voice. In these experiments the image of the larynx was reflected from a mirror placed against the soft palate, so as to be received upon a second mirror placed in front of the observer (auto-laryngoscopy.) An elementary knowledge of catoptrics will suffice to explain the principles upon which Liston-Garcia's method of investigation is founded. The examination itself is conducted in the following manner:—A metallic mirror—varying in size from six to fourteen lines in diameter, in shape either square with rounded edges, as recommended by Czermak, or oval, according to Türck's proposal, or, as it has been found very convenient by Dr. Levin, of Berlin, semi-circular, with a concave inferior margin—soldered to a slightly flexible metallic handle,—shown in the drawing half the real size—is to be introduced into the well-opened mouth

and fixed in such an angle against the uvula and soft palate as to throw incident luminous rays upon the larynx, and to reflect an image of the parts thus illuminated into the eye of the observer. To pre-

* This paper may be compared with one by Dr. Hardy, of Dublin, 'Retrospect,' vol. xxxix, p. 102.

vent the mirror from becoming dim by condensation of vapour upon its surface, it is necessary to warm it previous to introduction by dipping it into hot water or holding the unpolished surface over the flame of a small spirit-lamp. Garcia made use of the direct rays of the sun in his experiments: as this source of illumination, however, is not always available, and even, if so, attended with obvious inconveniences in practice, Czermak proposes the use of a perforated concave mirror of 7—12″ focal distance, by which the light of an ordinary lamp can be concentrated upon the larynx-speculum, the eye of the observer being applied to the perforation. As the distinctness of the image will depend upon the brilliancy of the illumination employed, it will be found advantageous to concentrate the light of the lamp upon the concave mirror, by means of a powerful bi-convex lens. Dr. Levin, of this city, has devised a highly-convenient apparatus for this purpose, consisting of a tin tube carrying a convex lens of two and a-half inches focal distance, and of about the same diameter, which, by means of a simple contrivance, can be fixed horizontally over an Argand lamp after the shade has been removed.

The perforated concave reflector can either be held between the teeth of the observer, fixed on a suitable ivory handle, as recommended by Czermak, or attached to a large spectacle-frame, according to Stellwag's proposal, or it can be suspended from a support screwed to the corner of the table on which the lamp is placed. The latter contrivance will be found the most convenient for practical purposes. I think it was first introduced by Dr. Levin.*

It will be most convenient to place the lamp to the right of the patient, who is to be examined in the sitting posture, his hands resting upon his knees, his body slightly advanced, and his head slightly reclining backwards. According to Professor Traube's advice, the lamp, concave mirror, and larynx-speculum ought to be on the same level, and the angle formed by the rays incident upon, and reflected from, the concave mirror as acute as possible. On this account it will be wise to place the lamp a little behind the patient. The observer supports the head and chin of the patient with his left, and introduces the larynx-speculum with his right hand, looking through the perforation of the concave mirror, by means of which he illuminates the pharynx.

By causing the patient to sound alternately the Roman vowels, *a*, *e*, the velum and uvula will be raised so as to admit of the mirror being introduced with greater facility. In pressing the speculum against the soft palate and uvula, great care must be taken to avoid touching the posterior wall of the pharynx, the palatine arches, and the base of the tongue to prevent the supervention of vomiting and deglutition. "In this manner" as Czermak says, "it is possible to look into the very depths of the pharynx, to obtain a distinct image

* Mr. Yearsley has requested us to state that he has used Mr. Avery's ear-lamp in this way for several years past.—Ed. of 'Med. Times.'

of the individual parts of the larynx, and, as I first demonstrated in my own person, to see the bifurcation of the trachea, reflected through the widely-opened glottis, with the tracheal rings, shining through the thin mucous membrane." p. 9.

Of course, considerable practice and a certain amount of dexterity is required for successful handling of the laryngoscope, notwithstanding the simplicity of the principle upon which the method is founded.

The difficulties are mainly owing to the great irritability of the palate, which in some indivduals is so considerable as not to tolerate the contact of a foreign body; others are unable to keep their mouths open for any length of time, or to command the position of the tongue, which ought to be well flattened and protruded. Some patients, as Professor Traube correctly remarks, suffer from a kind of "moral nausea," threatening to vomit as soon as they are told to open their mouths. This extreme irritability can be overcome by methodically accustoming the parts to the contact of foreign bodies, as it is often requisite prior to surgical operations on the palate. I remember reading that bromide of potash has the power of lowering the sensibility of the pharyngeal mucous membrane; it might deserve a trial in very refractory cases.

In general, however, the irritability of uvula and soft palate will be found very inconsiderable, so that they can be raised and pressed against the posterior wall of the pharynx without any inconvenience to the individual experimented upon. In Professor Traube's clinic I have seen an individual sitting for nearly ten minutes with the larynx-speculum applied to the fauces, so that fifteen medical men who were present could successively examine the reflected image of the glottis without any reflex phenomena supervening to interrupt the observations.

In this case the mouth of the patient was held open by a very convenient instrument, devised by Dr. Levin. The handle of the larynx-mirror is attached by a ball-hinge to the upper bar of the mouth-speculum, so as to admit of the larynx-mirror being easily adjusted for the purpose of demonstration.

In the fifth chapter, Czermak details his method for obtaining a view of the posterior surface of the velum, the naso-pharyngeal cavity, &c., and he represents the image obtainable by rhinoscopic investigation, the commencement of the Eustachian tubes being also rendered visible. Wilde has already investigated the latter by a similar method.

To obtain an image of these parts, a speculum must be introduced under the velum with its reflecting surface turned obliquely upwards, so as to illuminate the naso-pharyngeal cavity. A speculum is proposed for this purpose, to which a sliding wire-hook is attached, for the purpose of raising the velum.

Examinations of this kind are, of course, surrounded by numerous difficulties, and can only be expected to succeed if a combination of favourable circumstances obtains.

The auto-laryngoscopic observations instituted by Czermak for physiological purposes are mainly confirmative of the results obtained by Garcia's celebrated investigations, and his work will amply repay perusal to those who are interested in the important questions involved in the study of the mechanism of the human voice.

The pathological observations which conclude the work, twenty in number, illustrating most varied and interesting forms of laryngeal disease, as revealed by the larynx-speculum, are calculated to convince the most sceptical of the great advantages which must accrue to the practitioner from the adoption of this method of investigation.

The possibility of the eye serving as a guide for the hand in the topical treatment of affections of the larynx and deep parts of the pharynx is also proved by some of these observations. You must permit me to reserve my detailed statement for a future communication. Two of these cases—the first and third—during the course of which laryngotomy had to be performed, on account of stenosis of the larynx, are of particular interest, being the first in which by a novel adaptation of laryngoscopy the glottis was investigated from below. This was effected by introducing a small mirror attached to a suitably bent handle, with its reflecting surface turned upwards, into a fenestrated tracheotomy tube. By illuminating this speculum with a concave reflector the most brilliant and accurate images of the lower aspect of the glottis, &c., were obtained, and the nature of the pathological changes affecting the parts clearly ascertained. This method promises to be of great importance for the diagnosis and treatment of deep-seated affections of the larynx, particularly in cases of laryngeal tumours which cannot be attacked from above. By reversing the reflecting surface of the mirror introduced into the tracheotomy-tube, the deep parts of the trachea might also be explored.

I will conclude by stating, that those who wish to occupy themselves with laryngoscopy will do well to follow Türck's advice, and commence by studying, with an excised larynx, the outlines of the image reflected from the speculum, as the altered position of the parts in the reflected image is at first very puzzling to the unpractised observer. According to Czermak, auto-laryngoscopic exercises constitute the very best preparations for attaining efficiency in this branch of diagnosis. In plate 1, of Czermak's monograph an apparatus is figured, by which auto-laryngoscopy and demonstration can be combined.—*Med. Times and Gazette, May* 5, 1860, *p.* 455.

26.—ON DEMONSTRATIONS WITH THE LARYNGOSCOPE.

By JONATHAN HUTCHINSON, Esq., Assistant-Surgeon to the London Hospital, and Surgeon to the Metropolitan Free Hospital.

[This instrument seems to be exciting interest just now; we hope its popularity may continue, and that its meddlesome operations may assist us in applying proper remedies. Professor Czermak has been

showing how the interior of the glottis may be exposed to view, in different hospitals. We hope that it will not end in being *rum after*, like as was the speculum uteri and potassa fusa some years ago.]

The instruments employed were a round mirror for illumination, in the centre of which is a round hole—as in that of the ophthalmoscope—and a small reflector, mounted on a handle for introduction into the pharynx. The light must be either full sunlight or that of a moderator lamp in a dark room. Nothing is easier than by aid of the mirror to illuminate the pharynx. This being done, the next step is to introduce the small reflector, previously warmed, into the throat, gently pressing it against the uvula and velum. The patient must now be told to make slowly a prolonged *ah, ah, ah,*—in other words, to expire gently, so as to elevate the epiglottis. If this is done, and if the pharyngeal reflector have been properly placed, a full view of the interior of the glottis, true and false vocal cords, &c., is at once obtained.

Dr. Czermak exhibits the employment of the instrument on himself, and has acquired by long training, great dexterity in bringing the parts into view, and apparently entire freedom from all sense of irritation while doing so. He employs a second reflector, by which he is enabled himself to see exactly what he is showing to the spectator. Having introduced the mirror, he first showed the vocal cords, epiglottis, arytenoid cartilages, &c., in a state of rest, then emitting a variety of notes, made the cords narrow the aperture, vibrate, and go through their various evolutions with most astonishing rapidity and clearness. Having shown the closure of the orifice by the lateral approximation of the cords, he lastly brought the arytenoid cartilages and the base of the epiglottis into apposition from behind forwards, a feat which evidently required considerable effort to accomplish. Among those present at the London Hospital, were Mr. Luke, Dr. Peacock, Mr. Gowland, and many others, and all expressed themselves as highly satisfied with the clearness of the demonstrations. A like degree of gratification was, I understood, given to Mr. Paget, Mr. Bowman, &c., who had had private *séances*, and also at the other hospitals which the Professor visited.

Having concluded his illustrations of the state of the larynx in rest and in motion, Dr. Czermak next proceeded to show us the posterior nares and Eustachian tubes. This was accomplished by holding the soft palate downwards and forwards, while a reflector was introduced into the upper part of the pharynx. In this view the septum narium (its posterior edge), the opening into the posterior nares, the terminations of the middle and inferior turbinated bones, and the orifices of the Eustachian tubes were clearly seen.

Nothing could have been more instructive or more completely satisfactory than the Professor's demonstrations on himself, and he also succeeded very well on one of the spectators present. When, how-

ever, we came to cases of laryngeal disease, or what might be termed laryngoscopy under difficulties, the results were different. I had in attendance two patients on whom tracheotomy had been performed, and in whom we were exceedingly desirous, for purposes of treatment, to ascertain the exact state of the larynx. In the first of these, a man now under Mr. Luke's care, the operation was performed about four months ago, and he is as yet quite unable to breathe without the canula. The man has been a sweep, and it was thought within possibility that some growth of epithelial cancer might exist in the larynx. Although the man was patient, and did his best to assist the examination, yet his inability to accomplish a slow expiration defeated the Professor's attempts to obtain a view, since the epiglottis covered the interior parts. The second patient was a woman, the subject of tertiary syphilitic disease of the pharynx and larynx, for whom, about two years ago, I performed tracheotomy with much temporary benefit. A few weeks after the operation she became able to dispense with the canula, and has not since worn it. She has, however, had several attacks since, in which her laryngeal symptoms have been greatly aggravated, and she is permanently aphonic. The probability is that her rima glottis is much narrowed by cicatrices resulting from syphilitic ulceration. Her pharynx is also very much contracted by cicatrices, and this obstacle sufficed to entirely prevent the success of the Professor's endeavours to get a glimpse of her laryngeal structures.

It must be admitted that these two cases both presented unusual difficulties; nevertheless, they are those in which the aid of the instrument was most especially wanted. It is probable, however, that, after a few more trials, the patients might be so trained as to allow of a satisfactory examination. In Ophthalmoscopy the observer alone requires training; in inspecting the larynx, however, it is requisite not only that the manipulator should be dexterous, but that the patient should be able to assist him, both by tolerance and by accomplishing certain efforts which few at the first trial are able to do. By a little training the velum palati becomes much less irritable, and spasm is much less readily excited by the pressure of instruments. It is well known that training may do much in the way of preparing a patient to assist the operator in cases of staphyloraphy. Certain remedies, as, for instance, the bromide of potassium, and, possibly, the local application of chloroform vapour, may be found useful in inducing anæsthesia of the pharynx. By these it is very possible that the future application of laryngoscopy to practical medicine and surgery may be much facilitated. In chronic cases admitting of delay, and in intelligent patients, no doubt something might be done in instructing them to practise self-examinations by the aid of the mirror, and thus accustom the parts to the contact of the reflector.

Professor Czermak told me that he thought he had not inspected the larynx in more than 500 individuals altogether, most of these, of course, being in those suffering from disease of the part.

Out of this number, in no fewer than fifteen or sixteen he had been able to demonstrate the presence of polypoid growths upon it,—a fact of very considerable importance as regards the practical uses of the method. He stated that, with patience and time he did not believe that there would be more than ten per cent. in whom he could not succeed in getting the desired view.

The Professor has already left London on his way home, but it is in prospect, he informed me, that a pupil of his, Dr. Semeleder, should shortly visit us with the intention of remaining some time, and opening a class for instruction in the use of the laryngoscope. In the mean time, not a few will probably be anxious to try for themselves. The needful instruments (according to Czermak's patterns), if not already in stock, will, I believe, very shortly be so, both at Mr. Weiss's and Mr. Ferguson's.—*Med. Times and Gazette, Sep.* 8, 1860, *p.* 236.

27.—THE DIAGNOSIS OF PHTHISIS BY THE MICROSCOPE.

By Dr. Frederick James Brown, Rochester.

[This paper was read at the Gravesend Meeting of the British Medical Association; and the specimens exhibited were prepared by Dr. Andrew Clark, of London; they showed the presence of the elastic trabeculæ of the air-vesicles, and furnished evidence of the destruction of the lung-tissue.]

A few years ago, the pulmonary trabeculæ were discovered in the sputa in phthisis by Schrœder van der Kolk and by Dr. Andrew Clark, independently of one another. The former considers, or did consider, the examination of the sputa valuable only early in phthisis, whilst Andrew Clark holds that the examination is useful at all periods, as being the only *certain* means of detecting the disintegration of the lung and the increase of the disease. It is a fact that the disease often increases whilst the patient is getting fat, and appears sufficiently well to deceive himself, his friends, and his medical attendant.

The views of Dr. Andrew Clark, briefly expressed, are as follows:—

1. The parietes of the air-vesicles are indivisible; that is, they cannot be separated into layers. They are made up of membrane with bloodvessels, and yellow elastic tissue arranged as trabeculæ.

2. The air-vesicles are lined by scaly epithelium. The epithelium is easily demonstrated in the child; it is more difficult of demonstration in advanced life. It plays an important part in all chronic structural lesions of the lungs. It is absent in emphysema. The existence of an epithelium of the air-vesicles is denied by Bowman, Rainey, and Beale. They admit the appearance of cell-particles in the air-vesicles, but they believe them to be only the nuclei of the ca-

pillaries. They admit, also, the presence of nucleated cells that cannot be ascribed to the capillaries, but they believe them to be the product of morbid action. Dr. Clark's reply is, that no lung is found without them.

3. Previously to the tuberculosis there is a disorder of the assimilation, which may be termed pretubercular cachexia.

4. Phthisis commences with the deposit of tubercle, and is a term common to all the stages of pulmonary tuberculisation. Phthisis is the name assigned to a certain anatomical lesion of the lung, and is to be understood in a different sense to that in which the older physicians regard it; for with them phthisis is hectic fever arising from pulmonary tuberculisation. Phthisis, in the language of most practitioners, consists of hectic fever, due to softened tubercle in the process of elimination. This is symptomatic phthisis. Anatomical phthisis, so to speak, consists in the lesion of the lung, whether attended or not by hectic fever. The distinction resembles that made betwixt the pneumonia before Laennec's time and that pulmonary lesion recognised as pneumonia subsequently to the introduction of auscultation into practice.

5. Elimination of softened tubercle is the third stage of phthisis, and may go on in one part whilst deposition occurs in another; consequently, there is a mingling of the stages of phthisis—regarding this disease in an anatomical light.

6. Tubercle is a retrograde metamorphosis of the structural elements and of the fluid secretions (if there be any) of the part in which it may be found. Tubercle is not a deposit from the blood, but is a product of vitiated-nutrition—not a simple exudation, but a complex result of perverted vital action. When tubercular matter accumulates and presses upon the bloodvessels, it may excite irritation, in which case congestion is not unfrequently set up, which terminates in an exudation, which becomes mixed with the tubercular matter, and afterwards tuberculises. Such exudation in the first place is just like the exudation that takes place on an inflamed pleura: but the exudation being placed under unfavourable circumstances, does not become developed into a tissue, but degenerates into tubercle.

7. The air-vesicles are the seat of tubercle in the lung.

8. When the tubercular matter accumulates to such an extent as to compress the walls of the air-vesicles, the entire structure of the walls disintegrates, except the elastic trabeculæ (which are alone *characteristic* of the air-vesicles).

9. The elastic trabeculæ are seen in the sputa, lying in disorder amongst the disintegrated elements of the tissue, and are readily detected by the aid of the microscope.

10. The elastic trabeculæ are present in the sputa during the elimination of the tubercle, and constitute the diagnostic mark of the eliminative stage.

11. If elimination takes place with only minute deposit in the lung,

the microscope will reveal the disease sooner than the pleximeter and stethoscope; but where there is a considerable mass of tubercle in the lung, without elimination of any portion, percussion and auscultation, taken in connection with the general symptoms, will detect the disease, whilst the microscope gives negative results. Again, minute deposit in a quiescent state will be overlooked, although both the ear and the eye be employed for its detection.

12. The physician can confidently declare that phthisis exists when he finds the elastic trabeculæ in the sputa.

13. Phthisis commences with the deposit of tubercle, and tends to one of two issues:—(*a*) to recovery by absorption, by calcification, or by induration and pigmentary transformation; (*b*) or to suppuration and the establishment of hectic fever.

14. It is yet uncertain whether recovery or progressive disease be the more frequent.

15. The air-vesicles may remain filled with tubercle in a state of complete quiescence without softening or elimination, for years. This is a state of things that the microscope will not determine. It must be worked out by the study of symptoms, and of auscultatory signs.

16. The parietes of the air-vesicles remain intact in phthisis until one or other of the following events happen. (*a*) The tubercular matter accumulates to such an extent as to compress the walls of the air-vesicles with such force as to empty or obliterate the capillaries, when, nutrition being suspended, disintegration ensues, and the tubercular matter in one vesicle comes in contact with that in the adjoining vesicle, and so the two masses cohere. (*b*) The tubercular matter softens from any cause. (*c*) The tubercular matter sets up pneumonia, which breaks down the whole mass.

17. The elastic trabeculæ of the air-vesicles cannot be confounded with any other yellow elastic tissues *likely to be found in the sputa*, because of their character and arrangement.

18. Tubercle is often found in the tonsils; and the elongated cells of the tonsil-tissue, arranged as they are in an areolar form, might mislead an inexperienced observer. A little attention will obviate this apparent difficulty. The quasi-bands seen in sections of the tonsils consist of separate pieces, easily recognised as being separate, whereas the pulmonary trabeculæ are continuous and wavy. However, the distinction is made certain by chemical tests. The pulmonary trabeculæ resist the action of both acetic acid and liquor potassæ (inasmuch as they consist of yellow elastic tissue), whereas the elongated tonsil-cells resist acetic acid only, and yield to liquor potassæ, which renders them invisible.

19. The microscopic examination of the sputa enables us to detect softening tubercle in the process of elimination, which could not otherwise be detected; it confirms or corrects the diagnosis between phthisis, dilated bronchi, chronic bronchitis, &c.; and it determines the actual progress of the lesion of the lung.

Thus it is seen that the microscope confirms the opinion of the vulgar as respects "the spitting up of the lights," which is now an undoubted fact.

In conclusion, allow me to state my conviction that the detection of the yellow elastic tissue of the air-vesicles in the sputa is *one of the great discoveries* rendered to pathology by the microscope. The importance of being aware of the true state of matters in any given case, cannot be overrated; and I am sure that you will allow that when once we can pronounce a man to be under phthisis (using the term in the anatomical sense), we are likely to use hygienic and therapeutic means with greater precision and with greater prospect of success than if we considered the case to be one of catarrh, simple dyspepsia, or that *monstrum horrendum* "debility."—*British Medical Journal, April* 21, 1860, *p.* 302.

28.—ON THE IMPORTANCE OF THE FUNCTIONS OF THE SKIN IN THE PATHOLOGY AND TREATMENT OF TUBERCULAR CONSUMPTION.

By Dr. Toulmin, St. Leonard's.

(Being an abstract of a paper read before the Harveian Society.)

Dr. Toulmin commenced by offering as the proximate cause of tubercle in all cases, "the breathing of impure air, and air in so small a quantity as to render it impure, especially during the night." He observed that wherever this was the continuous state of existence, the result must be a deficiency in the red globules of the blood, and as the consequence of this, the deposition of plastic fibrin in an incomplete state of oxygenation, and, therefore, of organization, and thus incapable of being ultimately got rid of by change of matter. It consequently remained as an extraneous adventitious substance in the system, offering to the observer all the characteristics of tubercle. To explain the discrepancy which appears in the rich (who have no want of oxygen in the air they breathe) being equally subject to phthisis as the poor, he drew the attention of the Society to the importance of the respiratory functions of the skin, as proved by the almost instant death that occurs on closing the cutaneous pores by artificial means—as in varnishing and gilding the skins of rabbits and other animals; and observed, that in consequence of the coldness of our climate, and other causes, the higher classes of society were certainly not in the habit of making the washing the whole surface of the body a part of their daily toilet, and, consequently, that the exuviæ momentarily forming on the surface of the skin, the joint production of the sordes from within, combined with the *débris* of the cuticle, soon render the skin more or less impervious, although the individual might be in the habit of changing his linen daily. As an illustration of this state of skin, the author referred to acne, so frequently seen on the face, as

being in reality the general state of the skin of a large proportion of society, especially in the earlier periods of life, when phthisis generally shows itself; and that the free entrance of air, as well as the exit of carbonic acid through the skin, being thus impeded, the same imperfect oxygenation of the blood ensues as is produced in the poorer classes by breathing mephitic air. He remarked, that for the removal of this state of skin the only means were to be found in instituting a full and free diaphoresis by the aid of artificial heat, the result of which, in first softening, and then expelling, large quantities of inspissated sebaceous matter, after the surface of the body had been washed clean with soap and water, was surprising. The author drew the attention of the Society to the fact, that the use of the hot-air bath, as a therapeutic agent, was no innovation on the established practice of the profession, as it was the mode of bathing practised by Hippocrates, Galen, and Celsus, and that the universality of the practice was shown by the fact, that the remains of such baths had been found in every colony of the Roman empire.

The author enunciated some novel doctrines, both as to the nature and treatment of phthisis. For instance: If tubercle be imperfectly organized fibrin, then it should be looked upon as a blood-disease; and seeing it is found in other parts besides the lungs without destroying life, its deposition in them should not be considered as disease either of the lungs or air-tubes, but as an accidental circumstance, killing mechanically by its ulceration extending to the surrounding lung tissue. Again, he called in question the propriety of sending consumptive patients abroad to a warm climate during any stage of the disease, as, although in the later stages of the complaint when the air-tubes sympathized with the tubercular irritation, a warm atmosphere seemed more congenial to the patient's feelings, still in the earlier stages, when a cure was practicable, the breathing the open air of our winter (at least on the south side of the island) was most important; and he instanced as a proof that breathing cold air did not cause the complaint, the remarkable fact, that tubercular consumption is not to be met with in high northern latitudes.

The treatment of phthisis was considered under its hygienic and medical aspects. Under the former, and particularly in the earlier stages, the patient was recommended to live in a high, dry, and marine atmosphere, *on* the downs, rather than *under* them; to be as much as possible in the open air; to use all sorts of athletic exercises (avoiding such as accelerate the pulmonic circulation) suitable to the sex and strength of the patient, by which a more rapid change of matter is effected, together with absorption of already deposited tubercle, as well as the deposition of more healthy—i.e., of more highly organized, matter. Medically, the treatment was comprised in a few short aphorisms, the first and most important being,—Keeping the functions of the skin in healthy action by means of the hot-air bath. 2ndly. Anointing the whole surface of the skin daily with some olea-

ginous matter. 3rdly. Keeping a local ulceration always patent by means of an issue or seton. 4thly. The exhibition of some one or more of a large variety of tonic and antiseptic medicines—all admirable adjuvants in improving the general health, (if selected in conformity with the function most sympathizing with, and reacting on, the disease,) but powerless in arresting the specific lesion in question, without the previous "Open sesame" of the hot-air bath, followed by the aspersion of cold or tepid water. The author expressed himself as fearful of saying all that might be advanced in favour of this treatment in phthisis, seeing that the disease was universally considered incurable.—*Lancet, June* 14, 1860, *p.* 37.

29.—ON DYSPNŒA AS A MECHANICAL CAUSE OF CONGESTION OF THE LUNGS.

By Dr. Andrew H. Smith.

In the 'American Medical Times' for July 21, Dr. Andrew H. Smith has published an ingenious essay in explanation of the congestion of the lungs which results from severe or protracted dyspnœa. After referring to the theories of Haller, Goodwin, Bichat, Kay, Wintrich, and C. J. B. Williams, which chiefly attribute the impediment to the presence of venous blood in abnormal situations, he says that the degree of congestion capable of being explained by any of these theories may be estimated by examining the lungs of an animal destroyed by section of the medulla oblongata. In numerous instances in which he has performed this experiment, he has always found that, while some amount of pulmonary congestion was present, it was insignificant in comparison with that produced by occlusion of the trachea; although the chemical condition of the blood must be the same in the one case as in the other. He therefore argues that there must be another cause for the production of congestion by dyspnœa—and that this cause is to be found in the increased energy of the respiratory movements.

Dr. Smith observes that, in the mechanism of respiration, there are two avenues of ingress into the lungs—one by the air-passages for the air, the other by the bloodvessels for the blood; and that, in fact, we respire blood in precisely the same way and by the same mechanism as we respire air. The amount of blood drawn into the lungs in an ordinary inspiration is small, owing to the fact that the space acquired within the chest is readily filled out by air admitted through the trachea. But, obstruct the entrance of air, and at the same time increase the force of the inspiratory effort, and the amount of blood entering the lungs may be greatly increased. The atmospheric pressure upon the vessels within the thorax is rendered less than that upon the vessels of the body generally, and a rush of blood from every direction towards the thoracic cavity is the result. Hence in every

case of dyspnœa depending upon obstruction of the air-passages, there must be a congested state of the lungs at the termination of every inspiratory effort. That the congestion which has thus been produced during inspiration is not relieved during expiration, is due to the following circumstances:—1. The amount of blood to be expelled is increased; 2. The interval between the inspirations is diminished, so that less time is allowed for its expulsion; 3. The tonicity of the vessels is impaired by their over-distension, so that the expelling force is hereby diminished.

The following experiment has been performed by Dr. Smith to show the difference between the degree of congestion resulting from non-aëration of the blood, and that produced by the additional effect of increased energy of the respiratory movements. A ligature was placed around the trachea of an animal, and drawn so tightly as to prevent completely the entrance of air into the lungs. At the same instant that the ligature was tightened, the thorax was freely opened on one side, so that the air passed without obstruction in and out of the pleural cavity. By this means the influence of the respiratory movements upon the lung of that side was entirely suspended, while with respect to the other lung, it remained comparatively unimpaired. On examining the lungs after death, the one on the side which was opened was found to be but slightly congested, while the other was gorged with blood. The chemical condition of the blood must have been the same in both lungs, since both were equally excluded from the atmosphere. The effect of any change taking place in the action of the heart, or in the pulmonary vessels, must also have been the same in one lung as in the other.

Dr. Smith infers from the preceding reasonings, that in every case in which the respiratory movements are increased to any considerable extent in force or frequency (the entrance of air being obstructed), there must be a proportionate degree of congestion of the lungs as a necessary mechanical result.

The operation of this principle is well illustrated in a case of bronchitis. At the outset of the disease, the bronchial mucous membrane is congested, its vessels turgid with blood. This condition implies a thickening of the membrane and a diminution of the calibre of the tubes, and hence an obstruction to the entrance of air into the lungs. At the same time, the accompanying febrile movement quickens the circulation, and produces an increased demand upon the lungs. The respiratory movements become more frequent and forcible; and the expansion of the thorax not being responded to by a prompt and sufficient influx of air into the lungs, a compensating increase in the amount of blood flowing in is the consequence. The pulmonary vessels, already overloaded, receive a new supply; and their tonicity, already impaired by the effect of the disease, is still further diminished by this increased distension. With every increase of congestion, there follows an increase of obstruction, and therefore of dyspnœa, which,

in its turn, gives rise to a further increase of congestion. And so the process continues, the congestion aggravating the dyspnœa, and the dyspnœa aggravating the congestion, until finally the tension within the vessels arrives at such a point that a serous effusion takes place into the bronchial tubes, mingling itself with the products of inflammation. By this means the tension is relieved, and the actual stasis which would otherwise result is avoided. But if, as is often the case, the feebleness of the patient renders a sufficient expectoration impossible, or if the bronchial tubes, labouring under the disadvantage of having lost their ciliated epithelium, and of having their circular muscular fibres paralysed, cannot relieve themselves of the accumulating mass, then suffocation is inevitable, unless the process can be arrested by the intervention of art.

From the above considerations, and from the results of numerous experiments on animals and of a few upon the human subject, Dr. Smith has been led to believe that, in cases in which the cause of the obstruction cannot be directly attacked, the most direct and certain means of relieving the dyspnœa is by affording to the lungs an atmosphere which shall contain, in the volume which finds access to the air-cells, a quantity of oxygen equal to that respired in health. By this means the demand for oxygen is supplied, without the necessity for those violent muscular efforts which aggravate the cause of the dyspnœa, while at the same time they exhaust the strength of the patient.—*British Medical Journal, Sep.* 15, 1860, *p.* 726.

30.—OBSERVATIONS ON THE TREATMENT OF ASTHMA.

By T. L. Pridham, Esq., Bideford, North Devon.

Much has been written of late on the symptoms and treatment of asthma—perhaps the most painful and apparently alarming disease to which human flesh is heir.

The observations which I have to make on the disease are based on the experience of nearly one hundred cases which have come under my medical care. I may not throw much new light on the subject; yet I am desirous to record the result of my experience and my treatment, as well as my observations on the peculiarities of this most distressing complaint, in the hope that some little may be added to the stock of information which has already been collected on the subject.

It is now many years since a deep-rooted impression was made on my mind, by that memorable man, Abernethy, "that in order to treat disease, whether of a local or constitutional character, it was absolutely necessary to keep the stomach in that state, so that the food which was taken into it should be properly digested, in order to secure good blood, and consequently healthy secretions and healthy action of the body generally; and that in proportion as the surgeon or the physi-

cian attended to these particulars, so in proportion would the means placed within the reach of the practitioner be more or less successful."

With these impressions, which I believe to be most useful to the medical man, I started in my profession. I had not been long in practice when a severe case of what was called humid asthma presented itself for my treatment. I found that it had been of many years standing, and frequently seemed, from its violence, to threaten the life of the patient. The attack, however, would subside after a copious expectoration had been thrown off from the lungs; the man at the same time becoming almost exhausted for want of nourishment, which he was not for a time able to take, except in the smallest quantities. After the lapse of a few weeks, the man would rally, eat and drink largely, and go about his work until another paroxysm threatened, when I was again in attendance. On each occasion of an attack, I used to say, "This surely will be the last;" so severe and imminent did it appear.

This state of things went on for some time, till at last the thought occurred to me, How is it that asthmatic patients generally live to a good old age? They do not die of consumption; nor do the lungs undergo any other change except the dilatation of the air-cells, which dilatation of the cells does not much interfere with their normal action when free from an attack. Thus considering the matter, it appeared to me probable that the original cause of the disease was in the overworked powers of digestion; and I was borne out in this supposition by the circumstance that the patient had respite from attacks as soon as a certain amount of expectoration had been thrown off, and his stomach had for a season rested. Was it not then probable that impurities in the blood were formed by imperfect digestion, which were thrown off by means of the lungs? How far my ideas were correct, the following statement of treatment will, I think, go far to prove.

I speak now of what is called *dyspeptic asthma*—a disease which I believe to be peculiarly hereditary; that is to say, when the disease has once made its appearance, those who inherit it will ever be liable to attacks from imprudent diet; for I am convinced that an asthmatic person can never with impunity eat and drink as other people do. I may here state that, out of the number of cases which I have attended, I found hereditary predisposition in nine out of ten. And if it be true that asthma is an hereditary disease, I will endeavour, by the evidence of cases which I intend to publish, to shew that wherever it has existed in any member of a family for one, two, or even three generations back, exciting causes, such as imprudent diet, an attack of bronchitis, an attack of influenza, derangement of the liver, or any other functional derangement, atmospheric influences, peculiar odours, influences of the mind either for joy or sorrow, may at any time or period of life bring to light this peculiar disease, which has hitherto been latent in the system. It is fortunate that stomach-derangement

from imprudent diet, &c., is, as far as my experience goes, the most frequent exciting cause—consequently more under the control of medical treatment, provided the patient has resolution enough to second the efforts of the medical attendant in carrying out a strict system of diet and regularity; in fact, it may be said that the scales are thus held with disease and suffering on one side, and comparative health and comfort on the other.

Most of the other patients inherited gout, and in some instances both diseases were traced in the family; and in more than one instance where this was apparent, I found that there were alternate attacks of asthma and gout, which is, I think, an additional evidence that both diseases are hereditary, and may be considered blood-diseases. What the peculiar element in the blood may be which produces either asthma or gout, it is to be hoped scientific researches may one day decide.

To return to my first case. The man in question, possessing some good sense, although he gave himself to beer-drinking and gross eating, as he said, to get up his strength after an attack, after having heard my views of his case, replied, "Well, sir, your remarks appear reasonable enough; and I will endeavour to follow your advice, and not eat and drink so much." I wrote down a system of dietary and general management, which for a season he followed with advantage; after this, he became tired of the plan, and again fell into intemperate habits, with an increase, if possible, of all his original fearful sufferings. He at last became reduced in circumstances, and was obliged to apply for parochial aid. I thought I had now a good opportunity of trying the effects of diet with medical treatment on this patient, and I persuaded him to go into the union house. There I took good care that he should not deviate from my prescribed treatment; for I placed him under *lock and key*, ordering his food to be regularly given him, whilst he had a trustworthy attendant when he left his room for the purpose of air or exercise.

His appearance and symptoms at this time were as follows:—His countenance bore the signs of great distress. His shoulders were elevated; the eyes protruded; he had no appetite; the stomach was greatly distended after eating; pulse 70, feeble, yet regular; the tongue was coated, with fissures in it. There was great emaciation, and inability to lie down in bed or to walk up an ascent. The secretions from the bowels were dark. The urine was loaded, shewing an acid deposit.

After giving the patient an alterative pill and a saline aperient, I ordered him the following diet, which was to be regularly weighed out to him, and the hours of meals most strictly attended to. Breakfast at eight o'clock—half a pint of green tea or coffee, with a little cream, two ounces of dry stale bread; dinner at one o'clock—two ounces of fresh beef or mutton without fat or skin, two ounces of stale dry bread or well-boiled rice; three hours after dinner, half a pint of

weak brandy and water, or toast-water *ad libitum;* supper at seven o'clock—two ounces of meat, with two ounces of dry bread. He was not allowed to drink within one hour of his dinner or supper, or till three hours after; at other times, he was not limited. Open air exercise was ordered to be taken as soon as the office of digestion had been performed, but short of fatigue. In addition to this, I ordered him three grains of the extract of conium four times a day, at the hours of seven, twelve, five, and ten; the dose to be gradually increased to five grains four times a day. Under this treatment, in a few days the whole of his distressing symptoms subsided. At the end of a month, he was allowed three ounces of meat twice a day. At the end of two months, he had gained flesh and strength, so that he was able to follow his occupation as a carpenter, which he continued to do without intermission for some years, adhering strictly to the rules laid down for him, which he had now the good sense to adopt.

My views with regard to treatment in the case were simply these: not to give the stomach more to do than it could well accomplish; not to dilute the gastric juices by fluids before or soon after eating, but allow them to perform alone their proper functions; and to quiet the nerves of the stomach by means of a mild sedative, which prepared it for the due performance of the office of digestion. By this plan, the stomach soon began to crave for food, and a longing for the hour for taking nourishment appeared—good evidence that the powers of digestion were recovering.

Is it not reasonable, then, to suppose that a moderate quantity of food, well digested, supplies the system with more pure and nutritious blood than a large quantity of ill digested food can possibly do? It appears to me not unreasonable that the poison, as it may be called, which is generated by food ill assimilated, finds its way into the circulation; and hence arises the misery of hereditary asthma, which nothing but a paroxysm, and generally a copious expectoration will relieve.

I now proceed to the further evidence of cases which have been treated by me under the sedative and strict dietary system; and in doing so, I think I cannot give stronger proof of its efficacy than by quoting communications from patients themselves, as evidence of the relief they have obtained, and the life of comparative ease and comfort to them after adopting the system. But, before I go further, I would remark on the absolute necessity of procuring the best prepared extracts, which form of medicine I have invariably used for the purpose required. I have more than once been disappointed in my treatment, when my prescriptions have been sent to ordinary druggists, with whom sufficient care has not been taken in this matter; therefore I strongly advise medical men to procure their extracts from, or send their prescriptions to, some known first-rate practical chemist, whose reputation greatly depends on the purity of his chemical preparations. Genuine extracts retain for a long time their dis-

tinctive odour, remain moist, and do not mildew. The sedative extracts which I have generally used are those of henbane, conium, belladonna, and stramonium, gradually increasing their doses and frequently substituting one extract for another. as symptoms appeared to demand.

It is not my intention to enter into the more scientific department as to the supposed causes and effects of this mysterious and capricious disease, so ably treated of in the pages of this Journal by Dr. Hyde Salter, whose remarks I have read with much interest; my object is simply to record my remarks on the history of cases as reported by my patients, together with observations made by myself during my professional attendance, which may, I trust, lead to the mitigation of sufferings. My own opinion is, that it is impossible, from the present knowledge we possess, to come to the *real causes* of the disease in asthma; but still I trust practical observations may be valuable, inasmuch as they may lead to scientific researches, not only as regards the state of the blood in asthmatic patients, but also as regards the peculiar expectoration which is thrown off, through the medium of the lungs, in the different forms of the disease, which appears to me to be Nature's effort for relief; for it is my strong impression, as I have before stated, that it is in the blood that the secret lies, as, the more healthy is the state of the powers of digestion, so in proportion is the freedom of the patient from disease in dyspeptic asthma. Then, again, as regards persons being affected in certain localities, whilst perfectly free in others, may not the peculiar state of the atmosphere add impurities to the blood in some districts, whilst in others a more congenial atmosphere will lessen, and in some instances entirely remove, the impurity which exists in the circulating fluid in those predisposed to the disease by hereditary causes? I have, apparently, seen several instances of this kind, which I will hereafter name.

I will now proceed to record the history of cases of dyspeptic asthma.

Case 2.—In the year 1855, I was requested to visit a clergyman, upwards of 70 years of age, from the north of England, who had been residing in this town for some weeks, for the benefit of his health. I had frequently remarked him in the streets; and not knowing the exact history of his case, I considered he was in the last stage of consumption, so emaciated, so feeble and distressed, did he appear. I think I never saw any one walking at large so emaciated. I learned from him, that he had been afflicted with asthma for ten years, that during that time his clerical duties had been suspended, and that, in fact, he was quite a martyr. In this time of his affliction, he had sought every possible advice, both in London and in the country, but without any material benefit or relief; the general opinion given him was, that he would never recover, and that he must bear his affliction as best he could. He was not able to lie down in bed; and, for years, every night he had anticipated death before the morning came,

when, however, a copious heavy expectoration would with difficulty be thrown off from the lungs, and as the day advanced he became somewhat relieved, was then dressed, and passed the remainder of the day in great discomfort. Having inquired into his mode of living, I was told pretty nearly as follows:—At six in the morning, he had a cup of strong coffee; at nine he had tea or coffee, toast, eggs, or a chop; he lunched at one, generally on bread, cheese, and porter; he had a substantial dinner, with porter and wine, and a fair quantity of port wine after dinner; he had tea or coffee in the evening, as well as supper afterwards. My remark was, "And for all this amount of nourishment, you are as thin as any human being can well be." He replied, "I have been invariably told that, without such support, I I cannot live a month; and I take it to make up for the large amount I expectorate in the morning." I examined his chest, and found neither tubercular deposit nor any lesion of the lungs; but evidently, from the very clear sound emitted on percussion, the lungs were emphysematous, particularly under the right clavicle, whilst lower down a dull sound was emitted. In the morning, the pulse was seventy in a minute, but as night came on it rose to ninety or a hundred. His tongue had a very morbid coat—large, with fissures in it; his stomach was generally distended, and, though he ate so much, he had never any great desire for food; his bowels were irregular; the secretions dark; the urine was loaded with a pink deposit. At the conclusion of my first visit, I surprised him by saying, "You will considerably improve in health under the treatment which I intend to adopt, and, although your age is such, I should not be surprised at your one day being in harness again." His reply was, "That is utterly impossible; my disease has taken such hold on me for so many years." He further said, "he looked forward to nothing but death, and he often prayed to be released from his sufferings." I went on to remark, that if my treatment was to benefit him, seeing it was an extreme case of dyspeptic asthma, he must make up his mind to one thing, and unless it was fully carried out, he must not expect to derive any good result; that was, he must eat just one-fourth of what he was daily in the habit of taking, and, in addition, his food must be weighed, and taken as regularly as the clock struck. His reply was, "If that be your plan, you will soon shorten my days." I begged him to try it for one week, which he at length acceded to. I commenced my treatment on the 23rd of April, by giving an alterative dose of blue-pill, and a saline aperient. I then ordered three grains of the extract of henbane four times a-day, and the same system of dietary as prescribed in my first case, allowing him, in the form of animal food, only two ounces of it twice a-day. The result was, that at the end of a week he was able to lie down in bed and sleep several hours; his morning expectoration had lessened considerably; his appetite improved; his tongue cleaned; the distension at his stomach lessened; the urine was clear; his spirits lightened; and he could exert himself with

much more ease. He was now fully satisfied that the plan had so far succeeded; but his great fear was that he should be starved, by reason of so small a quantity of food being allowed him. At the end of ten days, I substituted the extract of conium for that of henbane, ordering him four grains four times a day. From this time, he improved rapidly until he left the town to return home, on the 14th May. On the 19th June, he wrote me from London, where he was staying with some friends: "I cannot be sufficiently thankful for the comparative ease and comfort which, through God's blessing and your skilful management, I have now enjoyed for the last six weeks. The weather has been most unpropitious, notwithstanding which, I have not entirely avoided the cold; my nose is rather 'stuffy' from its effects, but no symptoms of asthma have yet appeared, and I trust, by my strictly adhering to your regimen, I may be spared any return." On the 30th June, he wrote, from his home in the north of England—"I find the air here much keener than in Bideford or in London, and it has come chiefly from the east. I cough a little in the morning, on first waking; but I have felt nothing like asthma. I have adhered very generally to your rules, with only the addition of more bread to my breakfast and tea, which you have allowed me. My secretions appear all right. I am, as you saw me, almost a skeleton; but I am in hopes, however, that the wholesome food which I get here will be beneficial." I may remark here, that the months of May and June, in 1855, were unusually cold, the east winds generally prevailing. On the 12th July, my patient wrote: "I am perfectly free from asthma; my appetite is very good, and I greatly desire that my daily allowance may be increased; and the number of my pills, I think, may be reduced." I now ordered five grains of the extract of conium three times a-day, and increased his daily allowance of food a little, keeping strictly to the hours of taking it. On the 7th December, my patient informed me: "Notwithstanding the severity of the cold, and the dampness of the atmosphere, I am quite free from asthma. I now take my medicine only twice a-day; it appears to have a wonderful effect. I am able to lie down in bed all night; and I sleep six or seven hours at a time, and feel no difficulty in breathing, and I trust this winter I shall be able to remain at home without a return of my fearful disease." On the 17th Dec., he wrote to say he was still going on well—"Some days since, I imprudently exposed myself too much to the cold and damp, and was consequently seized with a severe shivering fit, and afterwards fever. I was confined to my bed for forty-eight hours; but no asthma made its appearance. I weighed myself last week; and notwithstanding my late attack, I found I had gained seven pounds in weight since the previous weighing."

Since the date of the last letter, I have occasionally heard from my patient, who still applies to me for advice whenever his complaint, from any imprudence in diet or irregularity, threatens an attack; and, to make my prediction true, I hear he has occasionally resumed his parochial duties as rector of his parish.

Case 3.—The third case of dyspeptic asthma was also in a clergyman, whose appearance was very different from that of the last, he being stout and lethargic. He came to be under my care from the county of Sussex in 1856. He had been afflicted for seven years, and his difficulty of breathing was constantly on him whilst awake, particularly after a full meal. On examination, I found his lungs greatly oppressed, and dull on percussion; his heart's action was very feeble and slow; his tongue coated and brown; the stomach greatly distended with flatus, which appeared to distress him greatly; the urine was loaded; he was unable to lie down in bed, and had heavy expectoration in the morning. He still performed his parochial duties, though with the greatest difficulty. Having cleared out the bowels by means of an aperient, I placed him on the sedative and strict dietary system, giving three grains of the extract of conium four times a-day, with a pill every other night of two grains of each of the extracts of taraxacum and simple alœtic pill.

He came under my medical care on the 1st September. On the 8th of the same month, I had the following *bulletin* from him. "My health improves rapidly under your mode of treatment, and I was able yesterday to repeat the responses at church with ease to myself; and I took a walk without feeling the least oppressed in my breathing or fatigue to myself. I have been afflicted with asthma for more than seven years, and have consulted several eminent physicians without anything like the relief which I even now enjoy. A week only has elapsed since I consulted you; and yet, I am thankful to say, my chest is so much relieved, that I am able to walk several miles without being obliged to rest in order to fetch breath." On the 18th Sept., he wrote:—"By this time you will be anxious to know how I am going on. The weather has certainly been very fine and clear since I commenced your mode of treatment; but many a year has elapsed since I have had such freedom from pain. On Thursday last, I walked up a steep hill of half a mile without stopping once; and on the following day, I bore the fatigue of a long journey into Sussex, without finding my chest at all oppressed. Last Sunday was the grand criterion; and I can assure you my voice was remarkably clear, and my chest but scarcely influenced by the enemy." On the 2nd October, he wrote:—"My health, on the whole, is very satisfactory. Since I last wrote to you, the weather has been very cold and stormy; notwithstanding which, I have breathed more freely than I have for many a year, and I now go about the active duties of my calling with confidence."

Another letter, a month afterwards, tells me he is quite well; since which time I have had no communication with my patient, but I learn from his friends that, thinking that he had remained so long free from complaint, he might with impunity return to his former way of eating and drinking whatever came in his way, he has consequently since experienced returns of his complaint, as might naturally be ex-

pected; for, as I have before stated, an asthmatic patient, who inherits asthma, can never with impunity eat and drink as other people; and although I gradually increased the amount of support taken in this case, yet the patient was not satisfied, but allowed his inclination to overrule his better judgment.

The above case must be admitted to be strong proof how much the disease is influenced by the manner in which the office of digestion is performed, and how much the patient has it in his own power to live a life of comparative ease and comfort.—*British Med. Journal, June 9 and July 28, 1860, pp. 434, 579.*

31.—*On the Essential Nature of Asthma.* By Dr. H. HYDE SALTER, F.R.S.—There are two ways (which I have not mentioned in my work), as indicated to me by my friend Dr. Brown-Séquard, in which bronchial spasm, when once established, may be kept up by the very conditions which it generates; one is, the power which carbonic acid gas possesses of producing contraction in smooth muscle. In asthma, the deficient standard at which respiration is carried on, and the diminished intercharge of the gases, produces an accumulation, in the air locked up in the air passages, of carbonic acid to an unusual degree. This, by the action to which I have just referred, sets the bronchial muscle still further contracting, and thus increases the very condition which at first caused the accumulation of the effete gas. In this way, asthma keeps up asthma. The other way is by the bronchial spasm stimulating the afferent or perceptive nervous filaments, and thus giving rise to reflex muscular contraction; just as the stimulation of the sensitive roots of the spinal nerves produces reflex muscular phenomena in the parts to which the corresponding motor nerves are distributed. In this way, muscular spasm becomes a stimulus to muscular spasm.

But in both these, as in the other ways I have indicated in my book, the bronchial spasm is secondary to an antecedent nervous condition.—*British Med. Journal, July 28, 1860, p. 589.*

32.—*On the Proper Use of Stramonium in Hay-Asthma.* By Dr. EDWARD LAWFORD, Leighton Buzzard.—[The thorn-apple or stramonium was first introduced to our notice from the island of Ceylon. Authors differ remarkably in their testimony as to its use—some reporting favourably, others considering it well nigh valueless.]

Smoking the herb is the plan generally recommended, and in those who are easily brought under its influence this plan is, doubtless, occasionally of considerable service; but with the majority little or no effect is produced by this mode of procedure. True, smoking, if long continued, may produce nausea and slight narcotism; but the paroxysm is urgent, and *immediate* relief is required. Under these circum-

stances, I have been in the habit of directing the patient to introduce portions of the stem and leaf of the plant into his pipe, as before, to smoke it, and inject the smoke thus obtained from the mouth into an inverted ale-glass; to continue this procedure until the glass is filled with the stramonium fumes, and then to place the glass (still inverted) over the mouth, and take one long and deep inspiration. The effect is instantaneous, a sense of suffocation for the moment, then copious expectoration of ropy mucus, and immediate relief.

Stramonium, in addition to its antispasmodic and narcotic properties, is a *direct, powerful, and instantaneous expectorant*, if properly applied, and in this consists its peculiar value in this distressing complaint; but in order to effect this object, it must be brought into *absolute contact* with the air-cells of the lung; simple imbibition through the mucous membrane of the mouth will not bring out its powerful expectorant properties, and hence those who have looked for benefit from smoking the drug simply, have been grievously disappointed, and have pronounced it valueless.—*British Med. Journal, Aug.* 18, 1860, *p.* 657.

33.—ON THE VALUE OF TONICS IN ASTHENIC DROPSY.

By Dr. R. S. Sisson, Cheltenham.

[It has been shown by Dr. Handfield Jones, that some cases of asthenic dropsy are curable by tonics alone, and are unaffected by diuretics. In the following paper Dr. Sisson shows that other cases, though benefited by drugs of the latter description, require the former to complete a cure.]

Mrs. ——, aged forty-five, has been ailing more or less for the last ten years, during which time she has been frequently under treatment with only temporary benefit. For the last three years she has had swelling of the face, abdomen, and legs. Catamenia have appeared only once in that time, and then in excess. Has had an eruption on the skin, unaffected by treatment. For the last two years or more she has lived on slops, forced down without appetite. Has had frequent vomitings, with diarrhœa, blood being often passed with the stools.

May 16th. Found her in bed, presenting the following symptoms:—Face much bloated; abdomen very protuberant; body generally anasarcous, with large dropsical swellings in depending parts; much emaciated in parts not concealed by effusion; patches of eczema on back of hands, feet, and front of legs; great dyspnœa; horizontal position intolerable; incessant cough, expectoration thin and frothy; complains of great pain in the chest; on percussion, marked dulness on the right side, over lower half of lungs posteriorly and anteriorly; loud crepitations; mucous râles; pulse 100, feeble; urine turbid, scanty, of dark-brown colour, not coagulable by heat and nitric acid;

stools liquid, dark, and offensive. To apply turpentine stupes to chest; chloride of mercury, three grains; powdered rhubarb, ten grains. To be followed by an aperient draught.

17th. Ordered bitartrate of potass, one scruple; vinegar of squills, spirit of nitrous ether, of each ten minims; compound decoction of scoparium, two ounces, thrice a day; beef-tea, and half an ounce of brandy every four hours.

The patient improved most satisfactorily up to the 25th, when her husband summoned me in haste, stating that his wife was dying. I found her gasping for breath; pulse at the wrist scarcely perceptible; had complained of great pain in the chest. Apply turpentine stupes to the chest; hot bottles to the feet, and give one drachm of brandy. Nutrients more frequently; continue diuretic mixture and brandy.

29th. The patient was delirious last night; got out of bed, and went into the adjoining room. This morning she is nervous and fidgety. Pulse 120, weak. Half an ounce of brandy every two hours; continue diuretic; twenty minims of tincture of opium at bedtime.

30th. Slept well last night; much improved; dropsy disappearing, except in face and legs; urine much increased, clear. She called my attention to a swelling in the right iliac region, for which she had been previously under treatment. Now that the ascites was removed, I perceived a tumour in this situation, of the size of a large fist; painful; movable; outline distinct; fluctuation perceptible. As vaginal examination was objected to, I did not press it in her present state, intending, however, to do so at a future time. To apply six leeches, and fomentation.

June 1st. Pain in iliac region removed. Apply six more leeches.

6th. At this time all the more urgent symptoms had disappeared, but the legs became enormously distended, and covered with one mass of petechiæ. The catamenia reappeared; excessive and pale. Discontinue the diuretic mixture. The legs to be bandaged. Ten minims of tincture of muriate of iron thrice a day.

From this time the patient recovered most rapidly, the bloated cheeks, however, remained unchanged, showing that the cure was not yet complete. Ordered to bandage the face tightly with handkerchiefs, and to continue the iron. In a few days my patient had the satisfaction of seeing her face reduced to its normal proportions. The eczema entirely disappeared, and by the end of June she had not only regained her strength, but stated that she never felt better in her life. I now pressed for an examination of the swelling in the side, when she assured me, that after a minute examination, not a vestige remained.

Here was a case of acute pulmonary œdema, supervening on chronic ascites and general anasarca, in a patient debilitated by long disease and inability to take substantial food. Her history was most unpromising, her brother having died of dropsy, her mother dropsical, and a sister, about whom I have since been consulted, in the same state.

To a young practitioner like myself, it may be supposed that the case was one of no little anxiety. But still, after a most minute examination, being satisfied that no organic disease existed, and believing in the wonderful power of drugs when properly employed, I predicted a perfect recovery. But when the ascites had disappeared, and my attention was called to the tumour in the iliac region, my favourable prognosis was shaken. Had I continued the routine practice with diuretics, not only should I have failed in effecting a cure, but I anticipated a return of all the symptoms which had disappeared. What diuretics failed in, iron accomplished, and has, I believe, rendered permanent a cure, which would otherwise have been of short duration. What would nature—what would the much-vaunted homœopathy have done in such a case? Allowed disease to have its sway, and hurry the poor patient with rapid and unerring strides to a premature and unnecessary death. Upon what pathological condition did the dropsical effusion in this case depend? Doubtless the more liquid portions of this greatly impoverished blood found a ready exit through the walls of the capillaries, relaxed from want of their proper stimulus—a healthy blood. The action of the iron is then obvious.

And now comes the most important question. What was the tumour in the iliac region? Could it be ovarian dropsy? Such was my diagnosis. But we are told that the contents of an ovarian cyst are never absorbed. The vaginal walls, however, may give way, and the contents be discharged by that channel. Was the supposed return, then, of the catamenia on the 6th of June such a discharge? I think it possible; but as a vaginal examination was not made, a doubt may exist whether the tumour was ovarian at all. Here is a tumour dating from a miscarriage ten years ago, gradually rising out of the pelvic cavity towards the abdomen, increasing as it proceeds; at times painful; movable; with defined outline and perceptible fluctuation. I ask, if not an ovarian tumour, what was it?

The extracts I have made of this case seemed to me as few as were compatible with the right understanding of so important a subject.—*Lancet, Aug.* 11, 1860, *p.* 132.

34.—ON PARALYSIS OF THE BRONCHIAL MUSCLES.

By Dr. JAMES F. DUNCAN, T.C.D., Physician to the Adelaide and Simpson's Hospitals.

The morbid changes occurring in the muscular tissue of the pulmonary system do not appear to me to have attracted that attention from pathologists which has been so readily extended to every other structure in the body. It is quite true, that we have for a very long period been accustomed to recognise spastic contraction of these fibres as a frequent cause of some forms of dyspnœa; and various appropri-

ate remedies have been resorted to for the relief of those diseases, suggested by the idea of their convulsive nature. But the opposite condition of impaired contractile power, if it has not been totally overlooked, has at least been greatly disregarded. No attempt, that I am aware of, has been made to ascertain the peculiar symptoms and signs arising from such a state, or to determine the treatment suitable for its removal or relief. And yet, judging from analogy, no reason can be assigned why such a condition as that of paralysis, or impaired vital energy, should not occasionally occur here, as well as in other muscles; or why, when such a condition does occur, its existence should not be discoverable by a careful observation of its attending phenomena.

Several circumstances naturally suggest themselves as serving to account for this omission. In the first place, the small size and obscure development of these muscles give us the idea that they are to be regarded rather as a rudimentary structure, preserving the analogy of form with the same parts in other animals, than as an apparatus designed for any special use in the economy of man. Very few practical physicians, I believe, look upon these muscles as performing any efficient part in healthy respiration; and hence they are not prepared to expect any material inconvenience to be produced as the result of their impaired action. In the second place, the fact that care has been taken, by the very organization of the parts, to prevent the respiratory process being materially interfered with by morbid changes in the vital action of these fibres, renders the occurrence of these changes, when they do take place, less obvious to the general observer. Not being voluntary muscles, neither the patient nor the physician is placed in a position to discover the loss of their contractile energy, while the manner in which they are associated with the elastic structures of the lungs is calculated to counteract the injurious effects that would be sure to follow, were collapse of the air-passages a necessary result of their paralysis. In the last place, morbid changes in the muscular structure generally are not of a nature to be recognised by microscopic examination. Changes in other tissues are too obvious to be overlooked. The mucous membrane may be congested, or thickened, or stripped of its epithelium. The serous membrane may lose its transparency, or be covered with an adventitious layer of lymph. The air-cells may be ruptured, enlarged, or otherwise altered. The parenchyma of the lungs may be hepatized, infiltrated with purulent matter, or œdematous, all of which can be easily recognised by the senses. But the muscular structure seems to defy alike the scalpel of the anatomist and the lens of the microscopist. Even atrophy, a condition, with the occurrence of which in other muscles every pathologist is familiar, becomes, in the instance before us, an uncertain phenomenon, to be discovered with difficulty, because the normal structure itself is so delicate that it seems scarcely possible to decide whether the appearance, in any particular case under examination, is really a morbid condition or not.

I have said that analogy would lead us to expect that occasionally, at least, these fibres should exhibit the condition of diminished vital energy, or paralysis. To Dr. Stokes the profession is indebted for having, many years ago, enunciated a pathological law, as regards muscular structure, which has been universally recognised since as a well-established principle. The principle is this, that when muscular fibres have been long exposed to a continued irritation from the existence of inflammation in an adjacent structure, those fibres which at first are thrown into a state of unnatural contraction, by contact with the inflamed parts, at last become paralyzed from over-stimulation. It was in this way he explained the bulging out of the intercostal muscles, which is universally observed in the advanced stage of empyema. But this principle, which is universally admitted, when applied to muscular fibres in contact with serous membranes, is equally applicable to muscular fibres in contact with mucous membranes. In fact, it has been so applied to explain appearances discovered in the dead bodies of persons who have died of ileus. Now, if this be true, and there seems no reason to doubt its accuracy, it follows as a matter of course, that long-continued inflammatory action in the bronchial mucous membrane ought frequently to impair, or even destroy altogether the contractility of the muscular fibres which lie in close contact with the under surface of the inflamed bronchi.

This idea was strongly impressed upon my mind by a case which I had recently under my care in the Adelaide Hospital, which presented some features of a peculiar character, that appeared to me to tally with what might be expected to follow from impaired energy of these fibres, and which derived immediate and obvious benefit from a plan of treatment adapted to such an hypothesis. Under the impression that this idea may be well founded, I venture to lay the particulars before the Association, in the hope that more extended observations may confirm the conclusions at which I have arrived, or, at all events, that the hints which I now throw out may lead to some practical improvements in the treatment of particular forms of chronic bronchitis.

Eliza Simpson, a married woman, forty years of age, the mother of nine children, was admitted into the Adelaide Hospital, December 12, 1859, labouring under a hard cough, with frothy and scanty expectoration, and suffering from intense dyspnœa. Her history was this: she was a laundress by occupation; twelve months previous to her admission, when in a state of perspiration, she had gone out of her washing-room into the open air, to gather in some clothes, when the weather was very severe, and snowing. In this way she got cold, which affected her breathing, and from which she had never recovered; within the last four or five weeks her condition had got very much worse, her sufferings being greatest at night; for several weeks she had been totally unable to lie down.

At the period of her admission she was a picture of suffering; she

was worn out from want of rest; looked pale and emaciated; her lips were livid, and her respiration, though only 18 in a minute, was forcible and laboured. On examining the chest, it was of a rounded form, hyper-resonant on percussion, with a feeling of increased resistance under the plexímeter, and with loud, sonorous, and cooing râles, audible in every direction. There was no lateral or antero-posterior enlargement of the thorax during respiration: it moved, as a whole, as if the ribs and cartilages formed one firmly united and unalterable piece, and the only increase its capacity admitted of was by the elevation of its upper margin, and the depression of the diaphragm.

The manner in which the respiration was carried on sufficiently accounted for the pain and difficulty she felt at night. The involuntary muscles not being able, without assistance, to maintain the necessary expansion of the chest to carry on respiration, each act of respiration was, to some extent at least, an effort of volition; and as the condition of sleep implies the abeyance of sensation, she was obliged to remain awake, to keep herself alive. But there was one feature in her dyspnœa which could not fail to strike the most ordinary observer, and which appeared to me to give it a peculiar character, and that was, the great disproportion which existed between the duration of inspiration, and that of expiration. In health, so far as I have been able to observe, the act of inspiration is really longer than that of expiration: at all events, I am satisfied that it does exceed in duration the period of time occupied by expiration. But in Simpson's case, this ratio was reversed, the duration of expiration being fully three times as long as that of inspiration. This could be seen by the eye, as well as felt by the hand, and heard by the stethoscope. I am well aware, that in chronic bronchitis, a prolonged expiratory murmur, attended by sonorous and sibilous râles, is one of the commonest and best known signs of the disease. But even in cases where the stethoscope reveals this sign, it is rare to see the difference manifested in a marked manner to the other senses, as it was in this instance.

How is this prolonged effort of expiration to be accounted for? I believe the usual way of explaining it is, by referring it to the diminished force with which the air is expelled from the lungs, as contrasted with that with which it enters; hence, when any mechanical impediments exist to the free exit of the air, owing to congestion of the mucous membrane, or the existence of the products of inflammation on its surface, it must take a longer time than usual for the accomplishment of the effort. But if this were true, how does it happen that such a phenomenon is never observed in acute bronchitis, where such congestion undoubtedly occurs, as well as in the chronic affection? Rejecting for this reason the explanation just given, I attribute the prolonged expiratory murmur to impaired action of the muscular fibres. So long as they retain their healthy vigour and contractility, weak as they are, they materially assist the act of expiration, by simultaneously diminishing the volume of the lungs; but when their

vital power is impaired, by reason of the long-continued irritation to which proximity to an inflamed mucous membrane subjects them, they no longer assist, or assist but very feebly and imperfectly, the other agents, whose office it is to diminish the capacity of the chest.

This, then, is what I would consider as the distinctive sign of paralysis, more or less perfect, of the muscular fibres of the bronchial tubes.

But to return to our case. She was treated on her admission with stimulating expectorants, and turpentine stupes at night, but with very little relief. A variety of expectorants were tried in succession, but without advantage. On December 24, she was put upon a mixture containing nitric acid, which agreed rather better with her than any she had previously used.

On the 26th the galvanic coil was used, and a current of electricity directed through the chest from the side of the neck to the pit of the stomach. This gave her no annoyance, but was productive of no benefit. On the 27th the report was:—"Looks a little better, but there is no material change; sleeps very little (all her sleep had to be taken in the sitting posture, or sleeping in bed with her head raised up); her breathing is very difficult." On that day she was ordered pills, containing half a grain of extract of nux vomica, and one grain of ipecacuanha, one to be taken three times a day, and her nitric acid mixture to be continued. The effect of this treatment was most surprising, and, I must confess, most unexpected. That night she was able to lie down and sleep all night; her breathing was much easier, and her expectoration free. She looked cheerful; and her appetite, which had been very deficient during the whole period of her illness, was improved. That was the first night, as she stated, that she had been able for seven weeks to lie down and sleep all night. Instead of broken snatches of rest in an uneasy posture, she was able, without inconvenience, to resume the natural recumbent posture, and to enjoy the refreshment that none can appreciate so well as those who have been long deprived of it. This treatment was continued with the happiest effects during the remainder of her stay in the hospital: with the exception that, on the 31st of December, the quantity of the extract of nux vomica was increased from half a grain to a grain for a dose: not because the former quantity had in a measure lost its effect, but under the idea that it might the sooner enable her to return to her employment. The effect of the pills, as she described them herself, was to relieve her breathing, promote expectoration, increase her appetite, and keep her bowels open. I certainly was not prepared to find nux vomica a promoter of sleep, believing, as I do, that it is in many of its properties an antagonist to medicines of the narcotic class. Yet so it was: the poor creature, who before was almost unable to close an eye, began to sleep in the day-time, to her own infinite relief. It is scarcely necessary to say, that I do not regard this result as contradictory in any measure to the physiological properties of nux

vomica. It was the consequence, not of its direct, but of its indirect action. It did not narcotize the sensorium, but it stimulated the muscular apparatus of the pulmonary system, and by so doing, removed a mechanical impediment to the proper performance of the respiratory functions. On applying the stethoscope to the chest, it was found that the bronchitic râles had lost much of their intensity; they were shorter and looser, and the expiratory murmur, as contrasted with the inspiratory, was less prolonged. Corresponding to this change in the phenomena of auscultation, there was a corresponding change perceptible to the eye and hand in the relative duration of inspiration and expiration. The former having lost somewhat of its voluntary character, was neither so forcible nor so short, whilst the latter, though still preternaturally longer, scarcely exceeded the former in duration. At the period of her discharge from the hospital, January 6, 1860, the two effects were very nearly equal in length of time.—*Dublin Quarterly Journal, May*, 1860, *p.* 299.

35.—*The Successful Treatment of Hooping Cough, by Increasing Doses of Sulphate of Zinc and Extract of Belladonna.* By Dr. FULLER, Physician to St. George's Hospital.—[In the last volume of the 'Medico-Chirurgical Transactions,' Dr. Fuller publishes a paper on the tolerance of belladonna by children. The following abstract of a paper read before the Harveian Society of London, contains a sufficient epitome of that article.]

Dr. Fuller commenced his paper by calling attention to the commonly-received opinion, as stated in most books on the subject, that, "Hooping-cough must run a certain course." He combatted this opinion by reference to the results of his own experience, and then proceeded to point out the causes which had led to its general adoption. Among these he mentioned the inefficiency of the treatment ordinarily employed, and the neglect of all measures likely to subdue the tendency to spasm on which the continuance of the whoop depends. Dr. Fuller regarded the complaint as consisting essentially of bronchitic irritation, usually not very severe, accompanied by reflex spasm of the air-passages, and expressed his opinion that in most cases the spasmodic symptoms are those which demand our most serious attention. Not only is the spasm a most painful symptom, but it is one which may give rise to life-long mischief. His object, therefore, had always been to subdue the spasm as speedily as possible, and the practice which, until the last twelvemonths, he had found most successful for the purpose, was the administration of sulphate of zinc in rapidly increasing doses. Dr. Fuller then referred to the discovery he had made as to the tolerance of belladonna by children, which is recorded in a paper published in the 'Medico-Chirurgical Transactions,' and he briefly stated the facts which are there detailed in *extenso*, as to the

amount of belladonna which may be given with safety, and the conditions which should be observed in its administration. The conditions he specified were, first, that the remedy should be given at least four times daily, and should be administered at first in small doses, which may be increased day by day or on alternate days, by a corresponding amount. He pointed out that mere dilatation of the pupil need not be regarded as a bar to its administration, and stated that if the precautions just referred to are observed, the daily dose of the extract of belladonna may be safely increased up to a scruple, or half-a-drachm without the production of any unpleasant symptoms. He then proceeded to state that he had brought these facts to bear on the treatment of hooping-cough, and from the conjoint use of sulphate of zinc and extract of belladonna, in rapidly increasing doses, had obtained results exceeding his most sanguine expectations. Rarely had he found the whoop to last above twenty-one days, and in some instances it had subsided in ten days. The mode in which Dr. Fuller proceeds is to give the zinc and belladonna as soon as the whoop declares itself. If the attack is accompanied by much febrile excitement, and bronchitic irritation, he prescribes a cough-drop containing a drachm of antimonial wine and a drachm of ipecacuanha wine to two ounces of water, and, if necessary, applies a blister to the chest. Of the cough-drop, a larger or smaller amount is given according to circumstances. In all cases, however, the zinc and belladonna are perseveringly administered. To children under three years of age he usually begins by giving one-sixth of a grain of extract of belladonna, and half-a-grain of sulphate of zinc four times daily; and to children above that age one-quarter of a grain of extract of belladonna and a grain of sulphate of zinc. These remedies are given in solution in water, and the dose of each substance is increased by a corresponding dose daily, or on alternate days; so that the child who began by taking one-quarter of a grain of the extract and one grain of zinc at a dose, would be taking one grain of the extract and four grains of the zinc at a dose, either on the fourth, the sixth, or the eighth day, according to the rapidity with which the dose is increased. Dr. Fuller concluded by citing cases illustrative of the value of this treatment; assured the members of its safety, and urged its general adoption.—*Med. Times and Gazette, May* 26, 1860, *p.* 539.

36.—*On Chronic Bronchitis and Emphysema.*—Of the number and variety of affections of the pulmonary organs which constantly present themselves for treatment at our hospitals, perhaps there is none so distressing, and so difficult to relieve effectually, as the above complication. Chronic bronchitis alone requires much attention and care, but when interlobular emphysema is combined with it the disease is quite incurable, and our efforts can be at best but palliative. Although this double form of chest disease is one of the peculiarities of the aged

who are subject to constant exposure, it is pretty commonly witnessed also in middle-aged and even younger persons; and in many instances wherein it is seen in apparently strong and vigorous individuals, the emphysema has been the primary affection, originating probably in rupture of the air-cells during forcible bodily efforts. To take a single example out of a number in illustration:—A hitherto strong and robust man (Nathan N.), aged forty-four years, a gas-stoker, was lately admitted into St. Bartholomew's Hospital, under Dr. Farre's care, with extensive emphysema and chronic bronchitis, which he has had for nineteen years. The suffering from dyspnœa and shortness of breath during that long period, in a strong man, may be readily imagined. Now, he was suffering from the consequences of this double malady; there was anasarca of the legs, body, and face; the prominent parts of his rounded back were swollen like puddings; the face was dusky, the lips blue, and the breathing very distressing. He was subject to epistaxis; the urine contained no albumen. In the treatment, attention was paid to the bowels; sinapisms were applied to the chest; cupping was resorted to between the shoulder-blades; he was ordered a mixture of decoction of senega, carbonate of ammonia, tincture of squills, and nitrous ether; and was besides put upon brandy. The improvement by treatment was but transitory, for he became moribund in the fourth week, and died shortly afterwards. As we learnt from Mr. Sprague, clinical clerk, there was strong pulsation in the external jugulars, and a systolic murmur of the heart, some days before death.

We might give many more illustrations of this malady even in St. Bartholomew's Hospital alone, but the present is sufficient for our purpose. Sometimes powerful medicinal treatment will do much in removing the urgency of the symptoms, but it never cures. And this leads us to ask the question, whether some relief might not be afforded by tapping the lung through the parietes of the chest (as we do in cases of thoracic fluid effusions), for the purpose of letting a certain amount of air escape, and so reduce the great pressure constantly exerted by the distended lungs upon the heart and its bloodvessels. At any rate the suggestion is worthy of remembrance in desperate cases.—*Lancet, July* 7, 1860, *p.* 8.

37.—*Pills of Carbonate of Ammonia in Chronic Bronchitis.*—By Dr. JOHN WILLIAMS, of Cork.—Dr. Williams recommends the following formula for administering carbonate of ammonia in pills, which, he says, are of great service in chronic bronchitis, and especially in those cases where the bronchial secretion is viscid and expectorated with difficulty:

℞. Ammoniaci, gr. ij; pulv. ipecac. gr. ½; morphiæ mur. gr. ⅛; ammoniæ carb. gr. ij; mucilaginis acaciæ, q. s. Misce. Fiat pilula.

The pills should be at once coated with a varnish of balsam of tolu, dissolved in chloroform, and they should afterwards be kept in a bottle.—*Pharmaceutical Journal.—British American Journal, June* 1860, *p.* 266.

DISEASES OF THE ORGANS OF DIGESTION.

38.—ON THE DIETETIC INVENTIVENESS OF HUNGER.

[Captain MARCY, in his interesting work 'The Prairie Traveller,' relate that when his party were yet eighteen days from the settlements of New Mexico, their supply of provisions, including tobacco, were entirely exhausted, and this when undergoing the severest labour.]

In this destitute condition we found a substitute for tobacco in the bark of the red willow, which grows upon many of the mountain streams in that vicinity. The outer bark is first removed with a knife, after which the inner bark is scraped up into ridges around the sticks, and held in the fire until it is thoroughly roasted, when it is taken off the stick, pulverized in the hand, and is ready for smoking. It has the narcotic properties of tobacco, and is quite agreeable to the taste and smell. The sumach leaf is also used by the Indians in the same way, and has a similar taste to the willow bark. A decoction of the dried wild or horse mint, which we found abundant under the snow, was quite palatable, and answered instead of coffee. It dries up in that climate, but does not lose its flavour. We suffered greatly from the want of salt; but by burning the outside of our mule-steaks, and sprinkling a little gunpowder upon them, it did not require a very extensive stretch of the imagination to fancy the presence of both salt and pepper. (There can be no doubt that the nitre and charcoal would to a great extent supply the want of salt. Soldiers and huntsmen have long resorted to this use of gunpowder under emergencies.) We tried the flesh of horse, colt, and mule, all of which were in a starved condition, and of course not very tender, juicy, or nutritious. We consumed the enormous amount of from five to six pounds of this meat per man daily; but continued to grow weak and thin, until, at the end of twelve days, we were able to perform but little labour, and were continually craving for fat meat.—*Lancet, April* 28, 1860, *p.* 435.

39.—*On the Absorption of Substances at the Stomach, and their Excretion in the Urine.* By Dr. LIONEL BEALE, Physician to King's College Hospital.—The rapidity with which weak solutions are absorbed from the digestive organs, and secreted by the kidney, is marvellous. In Mr. Erichsen's well known experiments, it was shown

that ferrocyanide of potassium could be detected in the urine within a minute after it had entered the empty stomach. These interesting conclusions were derived from experiments made on a case in which, from the deficiency of the anterior wall of the bladder and abdomen, the orifice of the ureters could be seen, and the urine collected as it trickled from them. A German suffering from this terrible malformation was in London two years ago, and I think many of you had an opportunity of seeing him, and observing how very soon after a large quantity of water had been swallowed, the rate of the flow of urine from the ureters increased.

Anything interfering with the absorption of fluid from the stomach or intestinal canal will necessarily affect the secretion of urine. In various cases where the contents of the alimentary canal are in a condition unfavourable for absorption, but a very small quantity of urine is formed. Dr. Barlow has gone so far as to say that the seat of an obstruction in the intestine can be ascertained by noticing the quantity of water excreted in the form of urine. When close to the pylorus, it is stated that scarcely any urine is separated. In ordinary cases of what is known as sick headache, where, from temporary stomach derangement, little absorption occurs for some hours, no urine is secreted perhaps for twelve hours or longer. The termination of the attack is marked by the very free and rapid action of the kidneys. —*British Med. Journal, July* 28, 1860, *p.* 576.

40.—ON THE USE OF CREASOTE IN THE TREATMENT OF DYSENTERY.

By Dr. W.T. GAIRDNER, Physician to the Edinburgh Royal Infirmary.

[Of the four cases of dysentery which occurred at the Edinburgh Royal Infirmary, under Dr. Gairdner's care, during a period of three months, three recovered quickly under the use of large injections of warm water, and moderate doses of opium, chiefly administered by injection.]

One case (James A., æt. 56) was exceedingly severe, and required a protracted treatment. The patient had been ill for several months before admission; he was very much reduced, and extremely pallid. The stools, no longer bloody to any considerable extent, were passed in small quantities at a time, with great suffering, and were extremely fœtid. I had great apprehensions as to the result in this case; for, from the slight effect produced by opiates and injections, and the severe tenesmus, with tenderness on pressure along the descending colon, I formed the opinion that considerable disorganization of the lower bowel existed. I determined, under these circumstances, to try a remedy which I have frequently found useful in subacute and chronic dysentery,—viz., the addition of creasote to the large emollient and detergent enemata, which I employ in all cases of dysentery whatever, and which I believe to be by far the most important element in

the treatment of that disease in all its forms. As I am not aware that creasote is in general use in the treatment of dysentery, I may state that I was led to its employment some years ago, by the statements of Dr. Wilmot as to its efficacy in an epidemic prevailing in the Union Workhouse of Pembury. I have employed it, however, somewhat differently from Dr. Wilmot, who administered it in large doses (℥i.) merely suspended in gruel. It appeared to me more likely to do good, and with less risk, if employed in smaller doses in solution, so as to be applied uniformly and certainly to the whole surface of the diseased mucous membrane. I have, therefore, usually employed the mistura creasoti of the Pharmacopœia, and have added one to six ounces of this, according to circumstances, to each large injection. It has been followed in several cases by the best possible effects; and I can safely recommend it as a valuable addition to the resources of the physician, in cases of dysentery attended by great irritation and fœtor of the evacuations. In the present instance, I employed a solution of creasote in glycerine,* which was prepared for me by the Messrs. Smith of Duke Street, to whom I had applied to find a neutral solvent for creasote, for another purpose. The relief afforded in the case of James A. was very remarkable. The injections were repeated twice or thrice a day, the dose of creasote in each being from ♏ v. to ♏ x., and the patient experienced so much benefit from them, that he proposed of his own accord to take the remedy by mouth also. The intense irritation subsided, and the stools became much less offensive. It is right, however, to remark, that the remedy appeared to lose some of its power after a time, and that this man was finally restored to greatly improved health under considerable doses of ipecacuanha, which was well borne by the stomach, and seemed to have a very decided effect in removing the remains of a very dangerous and exhausting disease.

In a case of obstinate diarrhœa from Bright's disease, and also in the case of John W., alluded to above, creasote was also tried as a *dernier ressort;* but only with the effect of showing that its good effects are probably limited to cases of genuine dysentery, accompanied by ulceration of the great intestine. It failed, in the others, to afford even temporary relief.—*Edinburgh Med. Journal, May* 1860, *p.* 994.

41.—*On the Use of Santonine in the Treatment of Lumbrici.* By Dr. J. Brisbane, Galashiels.—[In the treatment of ascarides, santonine is of little or no use, but Dr. B. says:]

I may mention, however, that I have used it in the lumbricoïdes with the most surprising, or I had almost said, specific effects. Much

* The proportions used were creasote 20 minims, to glycerine one oz. The materials require to be pure and to be well rubbed up together in a mortar. The result is what appears to me a perfect solution, though Mr. Smith doubts its being so. The subject deserves the attention of the framers of the New National Pharmacopœia.

depends on the method of administration. Before using santonine I had tried Dr. Watson's method of treatment by steel and purgatives alternately, with only trivial benefit. I also tried santonine by itself, and found it scarcely more efficacious than the former, the only advantage being that from its small bulk, it could easily be administered to children.

On resorting to the method suggested by Kuchenmeister of administering it with castor-oil, the effect was most gratifying. The first patient to whom I administered it in this way, a child fifteen months old (and who had been treated by the two former methods without benefit), passed no fewer than thirty-six worms, varying in size from a few inches to nearly eighteen inches. On repeating the dose several more were expelled. The effect on the child's appearance was really wonderful, from being a tiny emaciated creature, with a ravenous appetite, &c., it in the course of a short time assumed a plump, healthy aspect.

Since that time I have used it very extensively, and know many instances where from twelve to forty and even in one instance fifty-nine worms have been voided, and that, too, where Dr. Watson's method and the santonine *per se* had signally failed. Indeed I may say, where children have previously passed worms (which is considered the only unequivocal evidence of their existence), that I have never known the santonine and castor-oil together fail to eject more of them when administered.

I usually order from two to eight grains of santonine, stirred in from a tea to a table spoonful of castor-oil, according to age, to be administered at night and a dose of senna the following morning.—*Medical Times and Gazette, June* 9, 1860, *p.* 589.

42.—ON HYDATID TUMOUR OF THE LIVER.

By Dr. GARROD, Physican to the University College Hospital, &c.

[The liver is perhaps of all other organs the most liable to be infested by hydatids; luckily something can sometimes be done in these cases, whereas when in most other organs little or nothing whatever can be done.]

Assuming that an hydatid tumour of the liver has been satisfactorily made out, the question naturally arises in the mind of the physician, whether there is a single hydatid or a number of them present; for, unfortunately, it often happens that they exist in countless numbers, disseminated throughout the hepatic tissue. Dr. Watson in his lectures, mentions the case of a girl who died in the Middlesex Hospital, whose liver contained thousands of these globular bodies, the enlarged gland having, by its pressure, completely sealed up a portion of the inferior cava.

Experience proves, however, that it is seldom that more than three or four are developing in various parts of the organ at one time; but it is this circumstance which constitutes the difficulty, and we may say the danger, of interference; for if one is collapsed through evacuation of its contents, the others enlarge, and the patient dies at last from exhaustion.

Dr. Budd has afforded some useful hints on the treatment of hydatid tumours of the liver, which are of great practical importance. It is well known that hydatids sometimes spontaneously lose their vitality; the sac shrinks up, its inner surface secretes a peculiar matter compared to glaziers' putty from its appearance, and the tumour ceases to be of any further danger or inconvenience beyond that which arises from its mere presence. Now Dr. Budd proposes to imitate this state of things by art, if practicable, and he thinks it probable that it may be attained by medicine; but so far, the latter, except in a very few instances, has proved wholly unsatisfactory. He advises the tapping of the hydatid cyst through a *fine exploring trocar*, by means of an exhausting pump, drawing off all the water, and taking the precaution that not a particle of air shall enter the cyst—the danger of putrefaction of the dead hydatids being thus completely obviated. An instrument of the kind is prepared by Weiss, according to the plan recommended by Dr. Bowditch of Boston, U.S. Dr. Budd further advises the precaution of inserting the trocar downwards, with the outer extremity of the instrument at a higher level than the body, so that as the liquid rises in the canula it will expel the air from it. If the vitality of the hydatid is completely destroyed in this way, should others enlarge and become prominent, they may be submitted to the same plan of treatment if suppurative action does not spontaneously occur; should this take place, however, the nature of the case becomes changed to one of a more serious character. If there be but a single cyst in the liver, and no fresh growth make its appearance, a cure may be considered to have taken place.

The three examples of this affection which we record to-day were all tapped, but not with the precautions recommended by Dr. Budd; hence, in two, suppuration ensued; whilst in a third, at St. Thomas's Hospital, the sac had already undergone that process, and it was treated like a large abscess. In the following example the tumour was remarkably prominent just below the ensiform cartilage; it was tapped by a very fine trocar, and four ounces of aqueous and colourless fluid were withdrawn. This was followed by local peritonitis and rigors, which ushered in the suppurative crisis; and in three weeks from the first puncture an evacuation of pus was effected by a trocar. Afterwards matter with fœtid gas escaped from the old opening, and at the present time the former continues to flow to the extent of a few ounces daily. The general bulk of the liver has become diminished and the disease is now chronic; but it is most probable that there is only a single hydatid in this instance, although at first at was suspected that there might be more.

Jane J., aged nineteen, admitted March 23rd, 1860. Has enjoyed good health up to two years ago; the catamenia appeared at fifteen, and have always been regular until six months back, when they became irregular. Two years ago she had an attack of jaundice, which continued for four months, but did not confine her to bed. She states there was no tumour in the hepatic region at that time. After the attack of jaundice her health remained perfectly good again until last November, when she felt shooting pains in the back, under the right scapula, not extending down the arm, and at the same time discovered a swelling in front near the lower end of the sternum. This tumour gradually increased in size, and she soon felt an aching pain in it of a continuous nature. The patient thinks it was about two months after the discovery of the first that she noticed another small swelling beneath this. The swellings gradually enlarged, and became more painful, and at the same time her appetite began to fail. A month ago she was admitted into the hospital under Dr. Murphy's care, and the tumours have been painted over with compound tincture of iodine every day, without, however, diminishing them, or giving much relief. Since her second admission, another small swelling has made its appearance external to the other two. Bowels have always been regular.

March 27. Present state: Patient is thin and somewhat emaciated; muscles rather irritable; but in a better condition than on admission a month since. Hepatic dulness extends as high as the level of the right nipple, and below to horizontal line through umbilicus. From about three inches below the right nipple considerable bulging is observed, extending to the left, two or three inches beyond the median line. This bulging seems to be composed of two protuberances, the larger occupying the epigastrium and part of right hypochondriac region, the smaller below and somewhat to the left. No tenderness felt over prominent parts, but much below them. On pressure over the large central protuberance some sensation of elasticity is experienced, and sudden pressure on one side is propagated to the other. Conjunctivæ not turgid. On examining the back, some distinct but slight bulging is seen in hepatic region, and dulness on percussion extends about an inch higher on the right than on the left side. Slight curvature of the spine is noticed, the concavity towards the right side. She was put upon sarsaparilla, three grains of iodide of potassium, and decoction of cinchona, thrice a-day.

28. A fine trocar was passed into the centre of the larger mass in the epigastrium by Mr. Erichsen, when the suspicion of the hydatid nature of the tumour was confirmed; for a clear fluid (four ounces) was drawn off, which was found to contain minute flakes, each consisting of a collection of well-formed echinococci. After the tapping a compress was applied, and, with the exception of slight vomiting, no bad symptoms appeared.

30. Some pain and tenderness in the region of the puncture.

31. Much relieved from pain; tumour not much diminished in size, but the note over it is quite tympanitic.

April 1. Had an attack last night of rather severe rigors, with the return of pain over the swelling. To-day the pulse is 130, very weak; cheeks flushed; suffering great pain over the region of the liver on the slightest movement, deep inspiration, &c. Slight tympanitis of the abdomen.

3. Has less pain to-day; no return of rigors; still much tenderness over the hepatic region.

10. Pain has varied since the 3rd, but she feels much better; the central tumour appears larger and more tense.

19. Yesterday about six or eight ounces of thick fœtid pus were obtained by tapping the centre of the tumour. The tumour became flaccid for a time, but to-day is again distended. The complexion and the eyes are less yellow than they were.

24. On the night of the 22nd the tumour gave way from the old opening, apparently discharging air at first, then watery fluid, and afterwards thin fœtid pus. Yesterday the discharge of matter continued; but to-day it has ceased. The patient remains comfortable as before.

May 1. Since the last report the tumour has been discharging a mixture of pus and blood. The skin nearly natural in colour.

22. For the last few days the tumour has been again plugged up and very painful; patient much exhausted; pulse 130. Ordered a mixture of quinine.

24. The abscess discharged two or three teacupfuls of pus, after which she became better.

June 5. The tumour much diminished in size, and discharges pretty freely.

July 5. She remains much the same; very weak; unable to get up. Two grains of sulphate of iron to be added to her quinine mixture.

Aug. 2. She suffers less pain; discharge still continues, and amounts to about four ounces in the twenty-four hours; has not lost more flesh; the tongue is clean; and she is free from pain. She is picking up her strength, and is daily gaining flesh.—*Lancet, Sept.* 1, 1860, *p.* 310.

43.—ON SOME CHEMICAL QUESTIONS IN CONNEXION WITH DISEASES OF THE LIVER, AND THEIR TREATMENT.

By Dr. THUDICHUM.

[There are many points in this paper respecting practice, which we ought to attend to. Hitherto, affections of the liver have been treated very empirically. Calomel and blue pill, with some aperient, have

often been the sole remedies employed without any scientific object in view. In a former paper Dr. Thudichum referred to gall-stones, in the centre of which he had detected certain peculiar formations, which he thought were casts of the biliary ducts. This opinion has since been confirmed. Dr. Thudichum goes on to say:]

As matters now stand, essential constituents of gall-stones seem to be cholochrome, cholic acid, and earthy salts. In man, gall-stones contain a large amount of cholesterine in most cases; but that is a secondary and accidental ingredient, and is mostly crystallised around the other matters forming the nucleus. There are human gall-stones which, like those from the ox, contain no cholesterine. Others are said to consist entirely of cholesterine—a statement which requires to be verified by fresh analyses, which must prove the absence of cholic acid. Stearate and palmitate of lime are usually present in human gall-stones. The idea of an inspissation of mucus in the formation of gall-stones was shown by the author to be untenable; the binding material of the concretions is cholic acid.

The process of the formation of gall-stones was explained by Dr. Thudichum to be perfectly analogous to that which produces phosphatic calculi in the urinary bladder. The compound amido acids (taurocholic and glycocholic) split up under the influence of a cause of which the nature remains to be ascertained—probably a ferment from the intestinal canal. They deposit cholochrome and a portion of cholic acid, some salts, and a little fat, under the influence of acetic acid derived from glycocoll. In man, gall-stones contain cholesterine; this has, in the healthy bile, been held in solution in taurocholate of soda; but when the taurocholic acid is decomposed, the cholesterine is set free, and is deposited on any particles which may be at hand. In other words, the process of formation of gall-stones is the following:—

Healthy bile contains taurocholic and glycocholic acids, in combination probably with soda. Under the putrefactive change, these two acids split up; the taurocholic acid yielding taurine; and the glycocholic acid, glycocoll. Cholate of soda is left; the glycocoll is destroyed, and is transformed into acetate of ammonia. Valerianic acid is also formed, and combines with the ammonia from the glycocoll. Some free acetic acid then sets free the cholic acid from the cholate of soda, and the cholic acid is then precipitated in combination with cholochrome, so as to form casts of the tubes. This takes place in the ox; in man, cholesterine is also deposited in consequence of the breaking up of taurocholate of soda, which has held it in solution.

Within the last few weeks, Dr. Thudichum had examined the products of decomposition of fibrine, and had found amongst them cholesterine; which, with a gritty matter and an oil-like body, were left when all the matters soluble in water were removed. The presence of

cholesterine and gritty matter in atheroma led to the question whether this condition was not a simple result of the decomposition of fibrine or of albuminous matter from which the earthy particles had been washed out. It was further a question whether the cholesterine in gall-stones might not in part arise from the decomposition of cholic acid.

Dr. Thudichum then noticed that the healthy action of the liver-cells consists in the absorption of certain albuminous, fatty, starchy, and saccharine matters, and their transformation into the ingredients of bile. When this becomes impaired, the cells assume a fermentative tendency, leading to the same description of decay in the albuminous matter as that which is induced by putrefaction or by treatment by acids or alkalies. He then proceeded to point out that the general principles elucidated in his paper could be applied in the treatment of all liver diseases. In jaundice with obstruction of the biliary ducts, we should make early use of nitric acid or nitro-hydrochloric acid. He had with great benefit given a watery solution of nitrous acid. It is easily made, and may be drunk as a pleasant lemonade. It is less apt to produce griping, which, during courses of nitric acid and aqua regia, is not rarely complained of. This acid not only destroys and lixiviates the deposits of cholochrome and other amido-acids, but it also tones the digestive organs, and acts antiseptically. Nevertheless, a remedy to stay the abnormal action of the liver-cells we are yet in want of. Creasote is beneficial. In all cases where the disorder of the liver proceeds from the intestinal canal particularly, creasote, charcoal, astringents like gallic acid, and preparations of iron, particularly the tincture of the sesquichloride given together with some nitric acid, are highly beneficial. In a case of desquamation of the epithelium of the kidneys consequent upon chronic jaundice, he had exhibited this latter mixture with the most decided benefit.

Nitric acid was introduced into the treatment of these diseases by Annesley. Many have used it since, with variable success. Dr. Thudichum believed that it is frequently not borne because it is not sufficiently diluted. The more it is diluted, the quicker it is assimilated, and the more certain are its effects.

It might appear paradoxical that a free acid should travel through the blood, and act upon certain parts through tissues. We are, nevertheless, obliged to admit the possibility of that occurrence. Thus oxalic acid and many other acids have been proved to have passed through the blood uncombined, and have been found in the urine in a free state. Even allowing that blood so mixed with nitric acid might show a precipitate, there is no reason why a molecular precipitate in the blood, which is full of molecules of its own, should not again dissolve.

All liver diseases require the greatest attention to the intestinal canal. Mild purgatives, of salts or fruit, Pullna water, Seidlitz pow-

ders, water containing one per cent. of ordinary chloride of sodium and a little sulphate, are the best remedies. If stronger purgatives are required, the vegetable ones deserve preference. The fatal error of treating with mercurials the various disorders of the liver, he could not sufficiently deprecate.

The diet should be juicy, include fruit and vegetables, and eggs and meat. Starch and fat are always badly digested.

The skin must be well attended to. Baths, with some aqua regia, stimulate the skin and open the epidermis.

In true bilious attacks, with indigestion, a furred, sometimes yellow tongue, constipation, sickness and vomiting, headache, yellow appearance of face, and albuginea, he had found the use of blue pills mostly hurtful. Whenever he had been called upon to treat such an attack, his treatment had to deal with the effects of the biliosity and blue pills combined. In cases where such attacks, without previous excesses, become habitual, gall-stones are not rarely developed. I have known two cases end in phthisis. One, that of a gentleman, married, about forty years of age, is yet under observation. In many cases of phthisis, in which gall-stones are found after death, this habitual biliosity did not rarely precede the disease of the lungs. But phthisis may also produce gall-stone disease.

Lastly, he adverted to the use of soluble phosphates. He had made the observation that bile contains no small amount of phosphate of soda. Without this salt, the assimilation of the fats must needs be imperfect. Considering, therefore, that the absence of the phosphate may be hurtful, and that its presence never has been found to be so, he was inclined to give it a further trial. It should be given in the food, together with chloride of sodium, or in a dilute solution in water. Since he exhibited before this Society the action of phosphates upon fatty acids, with which they produce an emulsion ready for transition into the blood, he had thought of combining the acids of cod-oil with phosphates, in order to make this substance more generally digestible.

Liebig has long since shown that the most pleasant manner of giving soluble phosphates consists in the administration of beef-tea; but where this extract cannot be had in a quite reliable form, a weighed quantity of the ordinary phosphate of soda, say to the amount of half a drachm or a drachm per day, or more where it is liked and well borne, is preferable.

In bile, soda preponderates over potassa. In the substance of the liver the proportions are reversed. The relations of these two bases require further study. It is not probable that the one may substitute the other, and *vice versâ;* and hence the want of potassa may become a source of disease. It is known that this base prevails in the muscles, soda in the blood.

By thus attacking the liver from all sides, we shall soon succeed in obtaining a surrender of its secrets. This organ can in this way only be stimulated, while it is out of the reach of so-called medicinal stimu-

lants. Torpid liver, sluggish fever, and the like, may be useful practical denominations; but they are not based upon any positive evidence. We want much less cholagogues than we require remedies to appease, subdue, and change the activity of the liver. The organ is accused of much of which it is not guilty. It is by the mouth that most diseases enter; and it is from the insults received from the direction of the intestinal cavity that this patient and enduring organ mostly derives its detriment.—*Lancet, Oct.* 20, 1860, *p.* 388.

DISEASES OF THE URINARY ORGANS.

44.—ON THE TESTS FOR ALBUMEN IN THE URINE.

By Dr. LIONEL BEALE, F.R.S., Physician to King's College Hospital, &c.

The most important soluble substances present in the urine in disease, but absent in the healthy secretion, are, *albumen, biliary constituents*, and *sugar*. The clinical importance of these is so great, that they deserve special attention.

Albumen.—The occurrence of albumen in the urine has been regarded as a most important pathological fact ever since the connexion between dropsy and disease of the kidneys was established by Dr. Bright. Albumen, it need scarcely be said, is absent from the urine of healthy persons, although now and then it may be detected for a short period of time in the urine of individuals who are not suffering from any serious or permanent derangement of the health. The presence of albumen must always be regarded by the physician as a point of serious importance, although at the same time, *per se*, it cannot be taken as evidence of the existence of any organic lesion, unless it has been clearly detected from day to day for a certain time. Many of the causes which give rise to the escape of serum from the vessels in other parts of the body, independent of disease, will determine its transudation through the walls of the renal capillaries, and, as a matter of course, it will be found in the urine.

To recognise with certainty the presence of a substance in the urine having so important a bearing in the discovery and interpretation of certain morbid processes, as albumen, is obviously a point of the utmost importance to the practitioner. In the examination of the urine of patients, simple tests are at once applied, in order to determine if this substance be present or absent. In the majority of cases, such information is easily obtained; but occasionally an instance occurs in which, without great care, an erroneous conclusion is likely to be arrived at after the ordinary tests have been applied. As this question is one of very great practical importance, and of much interest, I propose to consider it at somewhat greater length than is usual in works devoted to the clinical examination of urine. The reactions

to which I shall refer are not imaginary, but have actually occurred; and I have known instances in which albumen was stated to be absent when the urine contained a large quantity; and other specimens have fallen under my notice, which, although they really contained none, yielded a precipitate having many of the characters of albumen. Let us then investigate the phenomena *seriatim*.

1. *Albumen is usually precipitated from its solution upon the addition of a few drops of nitric acid.* Heller recommended that the acid should be allowed to flow to the bottom of the tube containing the urine. In this manner, three strata are formed, the lowest stratum consisting of the pure acid, above which is the precipitated albumen, while the upper stratum consists of the fluid containing the albumen uncoagulated.

2. *Albumen is also generally coagulated by the application of heat* (140° to 167° Fahr.). If very dilute, a higher temperature is required. The best way of testing urine by heat is the following:—An ordinary test-tube is about half filled with the urine, and is to be held by the lower part. Heat is applied to a point near the *surface* of the fluid; the tube being shaken a little at the time, to prevent the glass being cracked. The slightest precipitate cannot fail to be observed, as the fluid below remains perfectly unchanged. When urates are present, this plan is very useful, as we get three distinct strata; the upper one formed of coagulated albumen; the next clear, in consequence of the solution of the urates at a temperature somewhat below that necessary for the coagulation of the albumen; and lastly, the unchanged urates.

3. *If the solution of albumen be alkaline, no precipitate will be produced by heat. We are, therefore, generally directed to neutralise the alkali by an acid before heat is applied.* If excess of acid be added, the albumen is, of course, precipitated in the insoluble form, without the application of heat.

Frequently specimens of urine are met with which exhibit one or more of the following peculiarities, which tend either to make us believe that albumen is present when it is not, or cause us to conclude that it is absent when the urine contains it.

1. Upon the application of heat, the specimen may become turbid, in consequence of the precipitation of phosphate. The reaction of the urine in this case would generally be neutral or feebly alkaline; but sometimes urine depositing phosphate on the application of heat is of a decidedly acid reaction.

2. Upon the addition of nitric acid, the specimen becomes turbid, in consequence of the decomposition of the urates held in solution in the urine, and the deposition of uric acid in a granular state. If the acidified urine be boiled, it usually becomes clear with the development of a pinkish or brown colour, consequent upon the decomposition of the uric acid.

3. Upon adding nitric acid to some specimens of urine of high spe-

cific gravity, an abundant precipitate of a crystalline character is produced. This consists of nitrate of urea, and is easily recognised by its crystalline character. It seldom appears immediately, and is hardly likely to be mistaken for albumen, except by a most superficial observer.

4. After adding a drop or two of nitric acid to urine suspected to contain albumen, in order to render it distinctly acid, *no precipitate is produced upon boiling, although a large quantity of albumen may be present.* This is constant in all specimens of albuminous urine, and shows the importance of never boiling urine suspected to contain albumen in a tube which may contain, by accident, a drop or two of nitric acid.

5. Cubebs, copaiba, and some other resinous substances, taken internally, are said to give rise to precipitates in the urine which are liable to be mistaken for albumen.

A Precipitate produced in Urine containing no Albumen.—Phosphate resembling Albumen. The precipitate of phosphates is very readily distinguished from albumen by its solubility in a little acid. Upon the addition of a few drops of nitric acid, the turbidity instantly disappears, and the solution becomes perfectly clear.

Uric Acid resembling Albumen. When a precipitate of uric acid in a minute state of division is caused in consequence of the decomposition of the urates by nitric acid, its nature may be ascertained by allowing the mixture to stand for some time, when the minute granules gradually increase in size, and at length become crystals, the nature of which is at once recognised upon microscopical examination. In some cases, the crystals may be seen to form under the microscope. This precipitation of uric acid on adding nitric acid often leads to mistakes, and albumen is stated to be present in urine which really does not contain a trace. Several cases of this precipitate have occurred to myself, and I have heard of many others in the practice of friends. It has happened in the wards of our hospital, that the precipitate produced by nitric acid has been inferred by the clinical clerk to consist of albumen, when subsequently no precipitate could be obtained. The fallacy was explained by careful examination afterwards.

Dr. G. O. Rees has met with urine affording this precipitate of uric acid on the addition of nitric acid in cases of typhoid fever. Most of the instances which I have met with occurred in cases of liver-affection. I extract two or three as examples.

Urine from a Patient suffering from large Hytadid Tumours of the Liver.—A small quantity of urine was filtered, and, upon the addition of a little nitric acid, a precipitate was produced. After standing a little while, this was examined by the microscope, and found to consist of minute crystals of uric acid. These were dissolved upon the application of heat; but, as the solution cooled, they were deposited again in the form of much larger crystals.

A specimen of urine exhibiting the same peculiarity contained excess of urea. Upon the addition of half its bulk of nitric acid, the mixture became nearly solid, from the formation of crystals of nitrate of urea. The deposit in this instance consisted partly of urate of soda.

Another example, of which I have kept notes, occurred in a man aged 49, suffering from *rheumatic fever*. The urine was acid, specific gravity 1027, and contained much urate of soda. The practitioner who first saw the case boiled a portion of the urine. It remained clear; and he said, therefore, that it contained no albumen. A physician afterwards tested a portion of the same urine with nitric acid; and, finding that an abundant precipitate was produced, affirmed that much albumen was present. The deposit produced by nitric acid was found, by subsequent examination, to be dissolved by heat; and, when a portion was examined in the microscope, its true nature was decided by the presence of numerous uric acid crystals.

No Precipitate produced in Urine containing Albumen.—Albumen not coagulated by heat when a little nitric acid is present.—Upon the careful addition of a drop of nitric acid, the precipitate at first formed when the acid comes in contact with the urine, slowly dissolves as it descends towards the bottom. Upon boiling this *acidified solution, no precipitate of albumen will take place.* Upon the further addition, however, of nitric acid, the albumen is precipitated.

This reaction has often led to mistakes. Not unfrequently albuminous urine has been poured into a test-tube which contained a trace of the nitric acid remaining from some previous experiment; and, upon boiling the mixture under these circumstances, no precipitate of albumen has occurred. If a few drops of a dilute solution of nitric acid be added to a portion of albuminous urine in a test-tube, and the mixture boiled, no precipitate will take place. In fact, the addition of a little dilute nitric acid will prevent the coagulation of albumen by heat. Dr. Bence Jones was, I believe, the first to explain this fact, in a communication to the editor of the 'Medical Gazette' (vol. xxvii, p. 289); and, so far as I can ascertain, no other explanation for the circumstance than that offered by him has been given. Dr. Jones thinks that the solution of albumen is owing to the formation of a nitrate of albumen which is soluble in a weak solution of nitric acid, even although boiling, but insoluble in a mixture of acid of moderate strength. Dr. Bence Jones has also shown that albumen is not always precipitated from very acid urine upon the application of heat. From some experiments I have made, however, I have been led to conclude that the above result depends upon the decomposition of the phosphates by the nitric acid, and the consequent development of free phosphoric acid in which acid albumen is freely soluble. This view has been confirmed by some experiments which I made sometime since on the subject, and which have since been many times repeated. A weak solution of albumen was treated with a few drops of chloride

of calcium, and afterwards with a little ammonia. After having stood for twenty-four hours, it was filtered. In this manner, any souble phosphates present were removed. The solution was then tested as follows :—

1. Albumen was precipitated by the application of heat, or by the addition of nitric acid, as usually occurs.

2. A very small quantity of dilute nitric acid did not prevent the coagulation of the albumen by heat.

3. After the addition of a few drops of phosphoric acid, the fluid no longer coagulated upon being boiled.

Some of the same solution as the above, which had not been treated with chloride of calcium and ammonia, afforded the same results upon the application of the tests as other albuminous solutions. A few drops of a weak solution of nitric acid, or a little phosphoric acid, prevented the precipitation of the albumen by heat. The addition of phosphoric acid to an albuminous solution, or a soluble phosphate and a little nitric acid, prevented the precipitation of the albumen by heat.

These results, therefore, led me to conclude that a trace of nitric acid prevents the coagulation of a moderately strong solution of albumen by heat, in consequence of decomposing the phosphates and setting free phosphoric acid, in which the albumen is soluble. When, however, excess of nitric acid is added, its action predominates over that of the phosphoric acid, and the albumen is precipitated.

At the same time, it must be admitted that there are several facts connected with the behaviour of weak solutions of albumen with acid, and under the influence of heat, which are not satisfactorily explained ; and forms of albumen having different reactions are from time to time met with. The whole subject requires further careful investigation.

Scherer describes a variety of albumen which is only imperfectly coagulated by heat. It is probable, however, that many of the peculiar reactions met with from time to time depend upon the presence of other substances dissolved with the albumen, rather than upon any peculiar properties of the albumen itself, or the existence of a variety of this substance.

Other Tests for Albumen.—Albumen is precipitated from its solutions by alcohol, alum, and many metallic salts, as those of lead, mercury, copper, and silver. Bichloride of mercury is employed as a test, and ferrocyanide of potassium precipitates a solution of albumen to which acetic acid has been added. These salts will, however, produce precipitates in solutions of other substances allied to albumen.

On Estimating the Quantity of Albumen in Urine.—The quantity of albumen varies much in different cases, sometimes amounting to a mere trace ; while, in other instances, a proportion not much inferior to that present in serum has been met with. In order to estimate the quantity of albumen, it is only necessary to add acetic acid, and heat the urine in a water-bath to a temperature of 194°, or until it boils. The precipitate is to be collected on a weighed filter, well washed,

dried, and weighed. The albumen always contains a small quantity of earthy salts, which are obtained by incineration. The residue must be deducted from the weight of the dried precipitate.

Albuminous Urine, from a patient with acute inflammation of the kidney. The deposit contained numerous granular casts, but no fat-cells were present; specific gravity, 1015; acid. The albumen coagulated by heat and nitric acid.

New Substance allied to Albumen. Dr. Bence Jones obtained a new substance allied to albumen from the urine of a patient (under the care of Dr. Watson and Dr. MacIntyre) suffering from mollities ossium. The urine was slightly acid; specific gravity, 1034·2 (*Phil. Trans.* for 1848, p. 55.) The deposit consisted of phosphate of lime, oxalate of lime, and cylinders of fibrine. Phosphates were precipitated by heat; but the urine was cleared by adding a drop of acid. No precipitate was produced by nitric acid; but, after being heated and left to cool, it became solid. The solid material was redissolved by heat, and precipitated again when the mixture became cool. On some days, the urine coagulated by boiling; on others, prolonged boiling produced no change. A specimen, which did not coagulate by boiling, was carefully examined. It was acid; specific gravity, 1039·6. It contained much urate of ammonia, phosphate of lime, and oxalate of lime.

The new substance was precipitated from the urine by alcohol, well washed, and ultimate analyses were made. It contained 1·09 per cent. of sulphur, and ·20 per cent. of phosphorus. This substance is the *hydrated deutoxide of albumen*. It was soluble in boiling water, and the precipitate produced by nitric acid was redissolved by heat, and it formed again as the mixture cooled. A similar substance occurs in small quantity in pus, and in the secretion from the vesiculæ seminales. The urine contained 66·97 parts per 1000 of this substance—an amount equal to the quantity of albumen in the blood. The patient was passing about 35 ounces of urine daily, which would contain upwards of 1000 grains, or more than two ounces of this new material.

Dr. Bence Jones recommends that this substance should be looked for again in acute cases of mollities ossium. He suggests that the reddening of the urine upon the addition of nitric acid might lead to its detection.—*Brit. Med. Journal, April* 21, 1860, *p.* 297.

45.—ON THE IMPORTANCE OF ALBUMEN IN THE URINE IN A CLINICAL POINT OF VIEW.

By Dr. Lionel Beale, F.R.S., Physician to the King's College Hospital.

In the majority of cases, in which the urine contains a very large quantity of albumen, and especially if the urine be of specific gravity of 1020 or higher, the inference will be that the case is an acute one, and that this large quantity of albumen has not been passing from the

kidney for any length of time. By far the majority of these acute cases recover if the patients are placed under favourable circumstances. In some instances, however, the circulation through the kidney becomes more and more obstructed; the urinary constituents accumulate in the blood and seriously impair the various actions going on in the body, and especially affect the nervous system; and death results, probably preceded by coma and sometimes by convulsions. Occasionally pus is found in considerable quantity in the uriniferous tubes. Sometimes the acute stage passes off, and the albumen, although it diminishes in quantity, does not entirely disappear from the urine, and the acute attack afterwards proves to have been the commencement of chronic kidney-disease.

If the quantity of albumen be small, amounting merely to milkiness or opalescence when heat is applied, or nitric acid added to the urine, and especially if the urine be pale and of specific gravity 1012 or lower, we should suspect that the lesion giving rise to the escape of the albumen was chronic, and would ultimately destroy life. If the proportion of urea to the other constituents of the solid matter were large, we should form a more favourable opinion than if the per centage in the solid matter were very much less than in health. In the latter case, a great part of the renal structure would probably be involved; but, in the former, there would be reason to think the disease had only affected a certain number of the secreting tubules. There are, however, some exceptions to these general statements. Patients have passed small quantities of albumen in the urine for many months, and it has afterwards disappeared. In other patients the progress of the disease is exceedingly slow. I have known a man pass urine of the character above mentioned for upwards of twelve years; and I believe that this might go on for twenty years, or even longer, the patient perhaps dying at last of some other malady. Under favourable circumstances, the life of patients suffering from certain forms of chronic kidney-disease may undoubtedly be prolonged for years; but it must always be borne in mind that such persons are more likely to suffer from exhausting influences, cold, fatigue, &c., than others; and an attack which would cause no alarm for the safety of a man previously in good health, might be rapidly fatal to them.

It is very important that the medical officers of Life Insurances should be aware that there are many instances of persons having chronic disease of the kidney, who are not themselves aware of it. Neither is there in many of these cases anything in the appearance or history of the person that would cause the physician to suspect the true nature of the case. The discovery of albumen in the urine, or of casts in the deposit, is sometimes the only point which leads the practitioner to a correct knowledge of the doubtful nature of the life. Now, if one of these persons experienced a severe attack of catarrh, his life might be endangered; and any exposure to cold would be very likely to set up acute inflammation of the kidneys, already impair-

by disease, and thus prove fatal. The only way to discover such a condition, is to institute a careful examination of the urine in every case; but to carry this out practically, it must be confessed there are many difficulties and objections. A microscopic examination of the deposit in the urine will probably throw much light upon the case, and enable us to diagnose the condition with much greater precision than is possible from a mere chemical examination. We cannot be too cautious in arriving at a prognosis in these cases. Every circumstance connected with the individual case must be carefully considered, and the state of nutrition, the progress the disease has made in a given time, the state of other organs, the constitution of the patient, his circumstances, temperament, &c., must be passed in review. Even with the greatest care, we shall seldom feel justified in expressing any but a very guarded opinion as to the probable duration of life.

Albumen is always found in small quantity in urine containing pus. We shall therefore meet with it in cases of inflammation of the pelvis of the kidney (pyelitis), and in cases of inflammation of the bladder and of the mucous membrane of the urinary organs generally. Whenever blood is present in the urine albumen is detected; for, if blood-corpuscles escape from ruptured capillaries, a certain quantity of serum must at the same time pass through the same apertures. The colouring-matter of the blood is sometimes passed in urine in a state of solution; but in this case also, a certain quantity of albumen is present.

Dr. Bence Jones detected albumen in the urine of a patient who passed spermatozoa. The urine passed in the morning contained spermatozoa and albumen. The evening specimen contained neither. On a subsequent examination, no albumen could be detected.

I have detected albumen in many cases of pneumonia during the period of hepatisation of the lungs. It is present, also, in the specimens of urine first passed after the period of suppression in cholera. In some cases of acute rheumatism with pericarditis it is observed; and it has been occasionally detected in continued fever. In puerperal fever it is often met with, and in puerperal convulsions it is almost constantly present. Dr. Lever found that it was absent in only one case out of fifty. The pressure of the gravid uterus is probably a cause of the albuminous urine met with in some cases of pregnancy, but it cannot always be referred to the same cause, for it occurs at an early period of pregnancy, when the uterus is too small to exert much pressure. Dr. Tyler Smith considers that it is to be accounted for by an influence exerted upon the nerves, in those cases in which it is not connected with organic disease. After intermittent fevers albumen frequently escapes in the urine. In cases in which any physical impediment to the return of blood in the emulgent veins or inferior cava exists, and in some cases of obstructed portal circulation, as in cirrhosis of the liver, traces of albumen may be detected in the urine. In anæmia, and in cases of dropsy depending

upon an impoverished state of the blood, albumen is often passed. Sometimes a large quantity of blood extractive matter is also present. After long continued hemorrhages, when dropsy occurs, we not unfrequently find albumen in the urine. In these cases it does not depend upon kidney-disease, but upon the state of the blood. Just as serum escapes from the capillaries of various tissues of the body, it is prone to transude through the renal vessels. Lastly, in persons who have suffered for many years from affections which produce alterations in the capillary walls, albumen may pass off in the urine; and towards the termination of exhausting diseases it is frequently present.

In those cases in which its presence depends upon obstruction to the circulation in the kidney, the impediment may be *functional or temporary*, or it may be *organic and permanent*. As examples of the presence of albumen depending upon temporary congestion, may be adduced certain cases of pneumonia and cholera, cases of acute dropsy, and of dropsy consequent upon scarlatina, with many others. Fatty degeneration and chronic nephritis may be brought forward as instances of structural disease, giving rise to such impediment to the circulation of the blood as to cause albumen to transude through the capillary vessels.

In the majority of cases, the vessels of the Malpighian tuft doubtless form the precise seat of the escape of albumen; but there are reasons for believing that albumen sometimes passes from the capillaries surrounding the convoluted portion of the uriniferous tubes, and in some instances from those in contact with the straight portion. ('Archives of Medicine,' vol. i., p. 300.)

In most cases in which albumen occurs in the urine, casts of the uriniferous tubes are also found; for with the serum a certain quantity of coagulable material transudes, and this becomes solid while it lies in the tube, of which it thus takes a mould, and entangles in its meshes any loose bodies, as particles of epithelium, &c., which may happen to be in the tube at the time. In the first series of cases alluded to, casts are often absent, or, if formed, they are perfectly transparent. On the other hand, where the structure of the kidney is altered, the casts often afford evidence of the nature of the lesion. We shall consider this part of the subject in the next lecture. Albumen is, however, often present without any deposit, so that for its detection we must rely solely on chemical tests.—*British Med. Journal. April* 28, 1860, *p.* 315.

46.—ON RENAL CASTS.

By Dr. Lionel Beale, F.R.S., &c.

In many cases of congestion, and in inflammation of the kidney, a spontaneously coagulable material is effused into the tubes, and coagulates there, forming a *cast or mould of the tube*, which is

gradually washed out by the fluid which is secreted behind it, and thus it finds its way into the urine, from which it may be easily separated for examination.

A cast is composed of a coagulable material which is effused into the uriniferous tube; and, becoming solid there, it entangles in its meshes any structures which may be in the tube at the time, and forms a mould of the uriniferous tube. The characters vary very much in different cases, according to the state of the tubes and the part in which the effusion of the matter takes place. Various substances are often entangled in the cast; and, by observing the character of these, we are often enabled to ascertain the nature of morbid changes going on in tubes at the time the cast was being formed. Great difference of opinion has been expressed with reference to the nature of the material of which the cast is composed. By some it has been termed fibrine; but the striated appearance always present in coagula of this substance is not found in the cast. Others have considered the cast was composed of albumen; but it is not rendered opaque by means of those reagents which produce precipitates in albuminous solutions. Not more than five years since, it was stated by two observers in France and Germany of high reputation, at least in other branches of scientific inquiry, that the cast really consisted of the basement membrane of the uriniferous tube. How such a statement could be made by any one possessing even a slight knowledge of the anatomy of tissues, it is difficult to conceive.

The transparent material probably consists of a peculiar modification of an albuminous matter possessing somewhat the same characters as the walls of some epithelial cells, the elastic laminæ of the cornea, the walls of hydatid cysts, &c. I think it not improbable that these casts of the uriniferous tubes may really be composed of the material which, in health, becomes converted into epithelial cells. In disease, this substance, perhaps somewhat altered, passes into the uriniferous tubes, and coagulates there. If this be so, the formation of the cast must result from depraved nutrition, and the material must pass into the tube from the intertubular capillaries—an idea which receives some support from the fact that occasionally casts are formed although no albumen passes into the urine. According to this notion, it is possible that a cast might be formed quite independently of any congestion or morbid condition of the Malpighian tuft; but, as a general rule, there can be no doubt that serum escapes as well as the coagulable matter, and albumen is found in the urine.

The diameter and general character of the cast will be determined by the state of the uriniferous tube at the time of its formation, as the researches of Dr. Johnson have indisputably proved. If the epithelium be abnormally adherent, the cast will be very narrow; if, on the other hand, the epithelium be removed, it will be of the width of the tube. Should the epithelium be disintegrating, the cast will afford evidence of the change. If in a state of fatty degeneration,

fat-cells will be entanged in it. In hemorrhage from any part of the secreting structure, blood-corpuscles are present; and when suppuration occurs, the cast contains pus-corpuscles. When the transudation of the coagulable material occurs in a tube to which the epithelium is intimately adherent, or in a tube whose walls are smooth, the cast will be clear and perfectly transparent. The import of all these different characters is fully discussed in the works of Dr. Johnson; and several interesting cases, under observation for a considerable period of time, will be found reported in Dr. Basham's work.—*British Med. Journal, July* 21, 1860, *p.* 556.

47.—ON MORBID CHANGES AFFECTING THE STRUCTURE OF THE KIDNEY.

By Dr. Lionel Beale, F.R.S., Physician to King's College Hospital.

In cases where the blood which passes through the kidney is unhealthy, and especially when it is loaded with noxious matter, as takes place in the majority of cases of long-continued beer or spirit drinking, the secreting power of the renal cells is gradually impaired. They lose their healthy appearance, and become much shrivelled, and at last suffer disintegration; while, in consequence of the germs having been destroyed at an early period of growth, their place is not occupied by a new generation. A complicated series of morbid changes in other structures of the kidney gradually ensues; and, in consequence of the blood being rendered still more depraved by the accumulation in it of matters which ought to be removed by the kidney, other organs are gradually involved.

In the kidney, it will be observed that the coats of the smaller arteries become much thickened, the capillaries shrink, while their walls become thicker and often granular. The quantity of blood distributed to the organ diminishes; and many of the capillaries, being no longer required, shrink and cease to transmit blood. The diameter of the secreting tubes decreases, while the basement membrane is thickened and more impervious. The whole organ becomes hard, and at the same time small and shrunken. This decrease in size takes place principally at the expense of the cortical or secreting portion of the kidney, as would be supposed. Such serious changes in the structure of an organ so important as the kidney necessarily affect the system generally, but these we must not now stop to consider. The Malpighian bodies also waste. The remains of many be seen without a capillary in them being pervious; and not a few of those which still exist are found to be so altered that they can hardly be recognised as Malpighian bodies at all. The greater part of the blood sent to the kidney passes into the pyramids by the *vasa récta*, and soon re-enters the veins, a small quantity being distributed to those tubes and Malpighian bodies nearest the pyramids. The diminished amount of urea, &c., present in the urine, is probably separated in

this latter situation; while a certain quantity of water, with a little albumen and the material of which the casts are formed, also escape in this situation, as well as from the straight part of the tubes.

Long before the disease has arrived at this stage, the urine will be found to contain a very small amount of solid matter, which consists principally of salts and extractives, with a very little urea.

By many pathologists these changes are explained by the effusion of inflammatory lymph, and subsequent thickening, condensation, and contraction of the so-called *matrix;* but it seems to me that all the appearances observed may be much more simply accounted for on the view that they depend upon depraved nutrition and wasting, than by resorting to the hypothesis of the inflammation of a structure whose existence has not been satisfactorily demonstrated, and which, if it does exist, according to its warmest advocates, only serves as a supporting tissue to the more essential elements of the gland-structure. It is very hard to see why such a tissue, which takes no active part in the changes going on in the gland, should be the starting-point of all the serious morbid alterations which occur. The idea, I believe, has arisen from a supposed analogy between cirrhosis of the liver and the so-called chronic inflammatory disease of the kidney. Cirrhosis was considered to depend upon inflammation, thickening, and subsequent contraction of another supporting fibrous tissue (Glisson's capsule), which was supposed to surround the lobules of the liver, and by its contraction to press upon the vessels ("On Cirrhosis of the Liver," 'Archives of Medicine,' vol. i., p. 118). For the origin of these morbid changes, we must look to the altered actions going on in the secreting structure, and not to inflammations of tissues of doubtful existence, which take no part in the nutritive operations or gland-functions. The conclusions to which I have arrived from my own observations, with reference to the nature of the so-called matrix in the healthy kidney, and the changes taking place in disease, are at variance with those usually entertained both in this country and on the continent. The discussion of this question involves the whole subject of areolar tissue and its corpuscles, and I therefore propose to defer it until this has been fully entered into in another place. For an admirable statement of the opinions generally held, with many original observations, I must refer you to a work by Arnold Beer, just published in Berlin ('Die Binde-Substanz der Menschlichen Niere'). The drawings accompanying this work appear to me rather rough. The engraver perhaps has misinterpreted some of the author's representations.

In other cases, the epithelium undergoes a very peculiar change, to which much attention has been given of late years. Fatty matter accumulates in the cells of the uriniferous tubes. The intertubular capillaries and those of the Malpighian bodies are also affected in a similar manner, and little collections of minute oil-globules may often be seen at intervals in their walls. This change often commences in a few of the tubes, and gradually extends until the whole organ is

affected. The kidney is in many instances much enlarged, while its colour has become very pale. Fatty degeneration, in many instances, is not confined to a single tissue or organ, but almost every part of the body is more or less involved.

Bright's Disease.—This term has been applied to all *morbid conditions of the kidney associated with albuminous urine.* Of late years many important characters have been made out, by which we are enabled to distinguish several diseases of the kidney essentially different from each other—different in their origin, in their progress, and often in the results to which they lead. Dr. Johnson has accurately described several of these morbid changes, and his researches have been confirmed by other pathologists. However, some physicians still insist that the different conditions above alluded to are merely different stages of one and the same morbid process. Let me ask you to examine carefully the small contracted kidney so commonly found in the bodies of old drunkards, with its rough puckered surface and diminished cortical portion, and contrast it with the large, smooth, and pale kidney, in a state of fatty degeneration, which is not unfrequently met with in young people not more than twenty years of age. The causes of these diseases are different; the conditions under which they occur are different; and, although the result is fatal in both, death occurs in a very different way, Their chemical characters are different; their microscopical characters indicate the occurrence of changes which are totally distinct. Again, the treatment required in the early stages of these diseases, when alone any benefit is likely to be derived from treatment, is different.

The divisions and nomenclature adopted by Dr. Johnson are the following: *Acute desquamative nephritis; Chronic desquamative nephritis; Waxy degeneration of the kidney; Non-desquamative disease of the kidney; Fatty degeneration of the kidney; Suppurative nephritis.* Dr, Johnson still supports the same classification, and opposes the theory held by some pathologists with reference to the *oneness* of Bright's disease. He has recently written a paper on this subject, which will be found in vol. xlii. of the 'Medico-Chirurgical Transactions.' Dr. Johnson says, with regard to the oft debated question, if large kidneys, at a subsequent stage of the morbid changes, contract, "The *rule* is that a large Bright's kidney remains large to the end and does not become a small one; and on the other hand, a contracted Bright's kidney does not pass through a previous stage of enlargement.—*British Med. Journal, July* 28, 1860, *p.* 576.

48.—ON THE TREATMENT OF ACUTE AND CHRONIC ALBUMINURIA.

By Dr. George Corfe.

During the last outbreak of cholera in London—namely, in 1854—many of its victims who recovered from the direful scourge were left

the subjects of more or less permanent uræmia. Some of these cases proved fatal, and the mode of death differed but little from uræmic poisoning in the ordinary forms of morbus Brightii or albuminuria. The progressive and fatal steps of the disease were, insidious coma, total and permanent suppression of urine, a small pupil and a hazy cornea, a peculiar flabbiness on handling the fleshy parts of the body, and well-marked fumes of muriate of ammonia were formed, as a mirror, moistened with hydrochloric acid, was made to receive the exhaled breath of the patient. This latter pathological feature is induced by the non-eliminated urea becoming converted in its elements to ammonia when it arrives at the pulmonary cells for exhalation, and is oftentimes a most valuable indication of the existence of uræmic poisoning. The frequent occurrence of albuminuria in hospital practice since 1854 has induced me to keep a record of many of the most interesting forms of this disease; and in watching the progress of the affection, I have been led to group certain distinct phenomena, which will enable us to form an accurate basis, and serve as a diagnostic mark between the acute and chronic stages of this intractable malady. Such a desideratum appears the more necessary when we reflect how different the treatment should necessarily be in each form, and that by a judicious management of the acute, the direful ravages of a chronic stage are thereby obviated.

It may be stated, then as a pretty general rule, that where the urine is scanty, the specific gravity variable from 1010 to 1020, the morning secretion (urina sanguinis) lighter than that of the afternoon (urina chyli), the albumen copious both by nitric acid and by heat, the tube casts abundant, the urine smoky and frothy, with or without oil-corpuscles, though the amount of dropsy be small, yet the disease is, under these circumstances, in its early and acute form.

On the other hand, a chronic stage may be readily diagnosed by the following physical signs:—The countenance has already become more or less cachectic; there is frequent nocturnal micturition; the albumen is scanty, perhaps altogether absent; the specific gravity rarely exceeds 1010, and is persistent in weight; its acidity is so faint that that it can scarcely be exhibited, and there is a strong tendency in it to pass into an alkalescent state under the smallest doses of alkaline remedies; the colour is of the palest straw. The skin assumes a faint but distinct shining appearance, as though a thin layer of collodion or gum had been painted over its surface. There are frequent attacks of frontal headache; tinnitus aurium, lumbago, and general prostration of the system are also sequences of the acute form of the disease. It is by no means a rule that dropsy should supervene in such a change of structure as that which the kidneys undergo in the chronic form of albuminuria; indeed, this symptom is more frequently absent, but usually present in the majority of acute cases of the disease.

The pathological appearances of the two forms, the tendency to inflammation of the serous membranes, the frequent fatal attacks of

renal epilepsy, and other cerebral disturbances from uræmic poisoning, have been pointed out by others, so that I need not dwell upon these phases of the disease. My special object is to give to the profession some of the results of a successful treatment of the acute and of the chronic form. Amongst several hundred cases which have passed under my notice in hospital practice, I cannot find from the notes on them that there was any connexion between this disease and scrofula. Dr. Bright considers the occurrence of tuberculosis with albuminuria to be "decidedly rare;" and on this account, perhaps, one may prefer the more frequent adoption of an antiphlogistic and powerful drastic treatment in the early stages, which would be inadmissible if experience and pathology led us to suspect a complication of tuberculosis with the disease.

In treating the acute stage, my experience justifies me in urging strongly the adoption of one of the two following lines of practice:—If the patient has had no previous attack, and has lived temperately, and was otherwise in good condition before the invasion of the disease, then I would recommend a small bleeding, so as to affect the pulse under the finger; and, after two hours, the exhibition of half an ounce of fresh-made infusion of digitalis every hour for three doses. Both the bleeding and the diuretic should be administered early in the day. After a pause of twenty-four or thirty-six hours, according to the condition of the vascular system, the same dose should be repeated, and even to a third and fourth repetition, followed on each occasion by copious draughts of "nitre-drink," the form for making which is simply to dissolve a drachm of the nitrate of potash in a quart of thin barley-water, and a little "foots" sugar may be added to remove the mawkish taste. I have witnessed the most extraordinary results from from this line of treatment. If the disease does not yield—that is to say, if the invasion of dropsy increases, the albumen is yet copious, the specific gravity high, the tube casts numerous, and the urine scanty, instead of resorting to that mischievous and unscientific mode of attacking the enemy by stimulating congested kidneys, already semi-inflamed, with saline diuretics (the evils of which practice by-the-bye I have too frequently seen displayed in a few days in the pathological theatre,) then a most valuable agent is to be found in the determined exhibition of elaterium, accompanied with daily dry-cupping to the loins, and a sudatorium at night. The form for exhibiting this drastic is the following:—Fresh extract of elaterium, half a grain; powdered capsicum, two grains; gamboge, three grains; soft extract of jalap, four grains; make two pills. One of these pills should be taken at six o'clock in the morning; the second at eight; and if no powerful drastic effect follows by nine a.m., a third dose may be given. The intense vomiting and purgation which ensue, it must be acknowledged, are very trying to the patient; but he is amply rewarded for his suffering by a subsequent relief to all his dis-

tress, so that he has oftentimes begged for a repetition of the pills before it was thought advisable to put him to a second trial, even when such an heroic treatment has been met by heroic suffering. I am convinced that the value of this drug is only appreciated by a few physicians; and even where it has been employed, the exhaustion consequent upon its action has deterred many practitioners from persevering in its use. But it ought to borne in mind, that under such treatment the patient should be allowed a liberal quantity of animal food in the form chiefly of strong broths, with chops and steaks, together with a pint, at least, of stout or good ale, and vegetables, &c., daily.

I remember one instance amongst many which Dr. Watson was in the habit of mentioning to his class when speaking of the use of this drug in albuminuria. He was summoned to attend in consultation a case of the above disease. "When I arrived there," he would observe, "I found the man overwhelmed with anasarcous swellings, his dyspnœa was most urgent, and there appeared to be no prospect of relief. However, I ascertained from the practitioner that he had tried everything but elaterium. I urged its use, and prescribed some to be given immediately. Four or five years after this occurrence I was again in consultation with the same practitioner, when he asked me if I recollected the case of albuminuria, and what I gave him, adding that it made a complete cure of the man. I referred to my private notes, feeling assured that the chief drug was elaterium, and so it proved, and this was the form I administered it in:—One grain of extract of elaterium, dissolved in an ounce of spirit of nitric ether; a small spoonful to be taken every hour until the bowels have been well opened."

I have frequently used the same form with excellent benefit in children affected with the disease after scarlatina. As soon as the system has been thoroughly brought under the influence of the above remedy, immediate recourse should be had to another equally valuable agent—namely, the tincture of the sesquichloride of iron in half-drachm doses, and the most elegant and efficacious mode of exhibiting it is in a glassful of seltzer water. Allusion has already been made to the injudicious practice of giving stimulating saline diuretics in this stage of the disease, and it has repeatedly happened that hæmatine and red corpuscles have been voided, and finally suppression of urine has followed the use of such drugs, whereas an early and a discreet use of the remedies alluded to has been attended by a complete alleviation of the worst form of the disease. As the attack becomes more chronic in its nature, I have seen marked benefit follow the use of the syrup of iodide of iron in half-drachm and drachm doses, together with a generous animal diet, and a moderate use of good beer or porter, to the exclusion of all spirits. Drachm doses of cod-liver oil after each meal has proved a valuable adjuvant in the treatment of many severe chronic cases.—*Lancet, May* 19, 1860, *p.* 489.

49.—ON THE TESTS FOR DIABETIC SUGAR.

By Dr. LIONEL BEALE, F.R.S., Physician to King's College Hospital.

The presence of grape sugar in urine is readily ascertained by the application of certain tests, and if moderate care be taken in the examination, the detection of this substance is not open to many fallacies.

Moore's test for grape-sugar consists in adding, to the urine suspected to contain it, about half its bulk of liquor potassæ. If sugar be present, the mixture becomes of a rich brown colour upon boiling, which increases in intensity if the boiling be prolonged. The brown colour of the solution is owing to the formation of mellassic or sacchulmic acid; glucic acid is also produced in the decomposition. This test, however, cannot be depended upon for detecting the presence of small quantities of sugar, because there are some other substances besides sugar which will cause the development of the colour in a slight degree.

Trommer's test.—Of all the tests which have yet been proposed, that originally suggested by Trommer, or some slight modification of it, will be found of the greatest practical value for showing the presence of sugar in diabetic urine, in clinical investigations. Trommer's test is applied as follows:—A small quantity of the urine, placed in a test tube, is to be treated with solution of potash, and a drop or two of a solution of sulphate of copper is to be dropped into the mixture. If sugar be present in any quantity, the precipitate at first formed will be redissolved, and the solution will be of a *dark blue* colour. If only traces of sugar are suspected to be present, one drop of the sulphate of copper solution will be sufficient. The dark blue solution is now to be heated to the boiling point, and if sugar be present, a pale *reddish brown precipitate of suboxide of copper* is immediately thrown down. Instead of boiling the mixture, it may be allowed to stand for some time, when a similar deposit will gradually subside. If the suboxide is only reduced after prolonged boiling, this cannot be taken as good evidence of the presence of sugar, for under these circumstances there are some other substances which will cause the reduction of the oxide of copper. Again, if the solution simply *change colour* by boiling, *without the occurrence of a distinct precipitate*, we must not infer that the change is necessarily due to the presence of sugar, for almost all specimens of urine exhibit this change. A flocculent precipitate of earthy phosphate, which always takes place, cannot be mistaken for the suboxide, as it is quite colourless, or of a pale greenish tinge. The reaction alone characteristic is the production of a brown or yellowish precipitate (varying in quantity according to the amount of sugar the urine contains), either after the mixture has stood for some time, or upon boiling it not longer than for a minute.

Modifications of Trommer's test have been proposed by Barreswil,

and others, the most applicable, however, according to Lehmann, being that of Fehling (Lehmann's 'Physiological Chemistry,' by Day, vol. i., p. 288. Cavendish Society). As this test is more easily applied than the sulphate of copper and potash, it may be well to mention its composition,—69 grains of sulphate of copper are to be dissolved in 345 grains of distilled water; to this solution a concentrated solution of 268 grains of tartrate of potash, and then a solution composed of 80 grains of carbonate of soda in an ounce of distilled water are to be added; the mixture may be poured into a 1000 grain measure, and filled up with water.

Barreswil's solution is composed of the following constituents:—

Cream of tartar	96 grains
Crystallised carbonate of soda	96 „
Sulphate of copper	32 „
Caustic potash	64 „
Water	2 fluid ounces.

In using these tests, it is only necessary to add about an equal bulk to the urine in a test tube, and then to boil the mixture. If sugar be present, the precipitate of suboxide occurs immediately. The application of this solution to the quantitative determination of sugar, will be considered under *volumetric analysis*. Trommer's, or one of the above mentioned modifications, will be found the most delicate test which can be used, when only small quantities of sugar are suspected to be present, and the tartrate of copper solution is applied as easily as the liquor potassæ test, while the results obtained from it are far more to be depended upon. If albumen be present, the reduction of the oxide of copper does not take place, so that in using the copper test we must ascertain that this substance is absent. Ammonia also dissolves suboxide of copper.

It has been shown that allantoin, creatine, and creatinine, have the power of producing a precipitate of suboxide of copper, like grape-sugar, and more recently, M. Berlin has proved that uric acid possesses to some extent the same property.

Some time since I endeavoured to ascertain the cause of certain anomalous results, which were sometimes met with in employing this test; and as these to some extent explain the discrepancies of different authorities with reference to the presence or absence of sugar in the urine in certain cases, it is well to allude to them here. The following results were obtained.

1. The precipitate of suboxide of copper was readily dissolved by acetic, hydrochloric, and nitric acids. It was also dissolved by ammonia.

2. The precipitate was insoluble in a solution of chloride of sodium, but was readily dissolved by a weak solution of chloride of ammonium.

3. The addition of a few drops of chloride of ammonium previous to boiling, entirely prevented the precipitation of the suboxide, the mixture retaining its greenish colour. Upon adding some solution of

potash, however, the precipitate of suboxide was produced, and ammoniacal fumes were given off at the same time. If a moderate quantity of solution of chloride of ammonium was present, the precipitate did not occur upon the addition of potash, even after very prolonged boiling.

4. If a drop of a very dilute solution of the chloride of ammonium was added to a pretty strong solution of sugar, and, after the addition of the tartrate, the mixture was boiled, no precipitate took place, but the solution became of a pale brown tint; the suboxide being immediately thrown down upon the addition of a few drops of a solution of potash, with the development of ammoniacal fumes. In the above cases in which no precipitate took place, it was ascertained that there was the usual excess of alkali present in the test solution.

5. A solution of oxalate of ammonia also prevented the precipitation of the suboxide, but a greater quantity of this salt than of the chloride of ammonium was required.

6. A neutral solution of urate of ammonia (artificially prepared) also prevented the reduction of the suboxide, and dissolved the precipitate if added to it. On carrying out this experiment further, it was found that the *precipitate of suboxide of copper was dissolved by urine containing an excess of urate of ammonia.*

7. A solution of grape-sugar in water was prepared, and by a preliminary experiment it was ascertained that, upon being boiled with the tartrate test, an abundant precipitation of suboxide took place.

To a portion of the precipitate of suboxide produced in this way, about a drachm of healthy urine, immediately after it was passed, and while yet warm, was added, and the reddish precipitate was instantly dissolved, forming a perfectly clear solution. Upon further boiling, a slight precipitate of phosphate took place. The suboxide, however, could not be precipitated by the further addition of potash and prolonged boiling.

8. Upon mixing a small quantity of grape-sugar with the same specimen of healthy urine, and boiling the mixture with the tartrate test, *no precipitate*, except a little phosphate, was produced. About half an ounce of the same mixture of urine and grape sugar was placed in a test tube, mixed with six drops of yeast, and inverted over mercury. The whole was then placed in a temperature from 70° to 100° for about twelve hours, at the end of which time the tube was found quite filled with gas, and all the liquid was expelled into the vessel in which it had been placed. The specimen of urine with which the above experiments were tried, was allowed to stand in a still place; and when it had become quite cold, an abundant precipitate of urate of ammonia was found to be present.

9. A portion of the aqueous solution of grape-sugar was mixed with a strong solution of urate of ammonia (artificially prepared), and then a certain quantity of the tartrate solution was added, and the

mixture boiled. The characteristic precipitate, or opalescence, was not produced, but the mixture became of a pale fawn colour. In a weak solution of urate of ammonia, the characteristic precipitate appeared after boiling the mixture for some minutes. *So that, although much sugar is present, the colour of the mixture may be merely changed to brown, and no precipitate whatever may take place.*

10. A solution of grape-sugar was treated with a drop of a dilute solution of chloride of ammonium, and boiled with the tartrate solution. The mixture became of a brown colour, but no precipitate occurred. Upon the addition of a few drops of solution of potash, the precipitate of suboxide was produced.

A solution of grape-sugar, treated with Trommer's test, according to the usual method, behaved in the same way, in the presence of chloride of ammonium, as when treated with the tartrate of copper solution; but in this case a greater quantity of the chloride was necessary, for when only traces were present, ammoniacal vapours were given off, and the precipitate of suboxide subsided, as before remarked.

From the results of the above experiments, the following conclusions with reference to the practical application of Trommer's test, and Fehling and Barreswil's solutions, and other modifications of the copper test, may be drawn:—

1. That if the urine contain chloride of ammonium (even in very small quantity), urate of ammonia, or other ammoniacal salts, the suboxide of copper would not be precipitated if only a small quantity of sugar were present.

2. That unless there be a considerable quantity of one of the above salts present (in which case the blue colour will remain), the mixture will change to a brownish hue upon boiling, but no opalescence or *precipitate* of suboxide of copper will occur. When only a moderate amount of sugar is present, I have been unable to obtain a precipitate, under these circumstances, by the addition of potash to the solution, and prolonged boiling. By observation 8, it appears that a specimen of urine exhibiting this reaction may contain a large quantity of sugar, as ascertained by the yeast test.

3. That in many cases in which the precipitation of the suboxide is prevented by the presence of ammoniacal salts, the addition of potash to the solution, and subsequent boiling, will cause the production of a precipitate with the evolution of ammoniacal fumes. Hence care should always be taken that there is a considerable excess of free alkali present.

4. When only small quantities of sugar are present in the urine, and the precipitate of suboxide of copper is not decided, the fermentation test should be resorted to.

Upon treating different specimens of diabetic urine with Trommer's test, or its modifications, it has often been noticed that in one case the precipitate is produced as soon as the mixture reaches the boiling point, or even before; while in other instances it is necessary to keep

it in active ebullition for some minutes, before any precipitate is produced. This circumstance receives explanation from the facts above detailed with reference to the presence of ammoniacal salts; and other anomalous results, which must have occurred to many in the habit of employing this test, become explained.

As a test for diabetic or grape-sugar, if proper precautions be observed, much greater dependance can be placed on this test than by simply boiling with liquor potassæ. Specimens of urine in which sugar is suspected to be present, and no decided precipitate of sub-oxide (which must be carefully distinguished from phosphate) occurs, should be carefully fermented with yeast before any conclusion is arrived at.

On Testing for Sugar when only Traces are Present.—In fluids which are suspected to contain only mere traces of sugar, it is necessary to separate some of the other constituents before applying the test. The plan recommended by M. Leconte is the following:—Excess of acetate of lead is added, and the precipitate separated by filtration. The solution is concentrated by evaporation, treated with ammonia, and again filtered. The copper test is then applied. The objections to applying the reduction test to solutions containing ammonia has been already discussed. It is better to employ carbonate of potash, or soda, instead of ammonia. The excess of lead salt may also be removed from the filtered solution by passing sulphuretted hydrogen. The precipitate of sulphuret of lead is removed by filtration, and the liquid, after evaporation to a small bulk, may be tested.

Another plan recommended by M. Leconte is to treat the urine with acetic acid, and evaporate it to about the fifth of its bulk; it is then treated with alcohol, and after filtration from the salts, &c., the alcoholic solution is evaporated and tested. This plan is free from the objection that ammonia may cause the destruction of the sugar where only traces are present.

The Yeast Test.—This is one of the most satisfactory tests for the presence of sugar, and if employed with proper care can hardly fail in its results. Two test tubes, of the same form, and of equal size, are to be taken. One is nearly filled with water, and into the other a corresponding quantity of the urine is to be poured. An equal amount of yeast is now to be added to the liquids in the tubes, and after pouring in just sufficient fluid to fill the tubes, the thumb is to be carefully placed over the opening, and the tube inverted in a small cup of mercury. The best plan, however, is to cut out a little india-rubber pad, slightly larger than the upper extremity of the tube. When the tubes have been filled up to the brim with a little water, the pad is allowed to float on the surface, next a little cup or beaker is inverted, and carefully placed over the end of the tube. The india-rubber being pressed against the open end, the fluid is prevented from escaping, The whole may be inverted, and a little mer-

cury having been poured into the beaker, the india-rubber may be removed, with forceps, without any escape of the fluid. The tubes may be supported in position by a wire stand. Both tubes are then to be exposed, for a few hours, to a temperature of from 80° to 90°, and the comparative size of the bubble of gas in the upper part of each may then be noted. If an appreciable quantity of sugar be present, the bubble of gas in the tube containing the urine will be many times larger than that in the tube which contains the yeast and water. In the latter tube the bubble of gas merely arises from the small quantities of air previously mixed with the yeast, becoming disengaged, and floating to the surface. Fermentation, when carefully performed, is positive evidence of the presence of sugar, although it does not indicate the kind of sugar present.

Maumené's Test.—A little woollen rag, as merino, is cut into strips, and soaked for four or five minutes in a solution of perchloride of tin (one part of the perchloride to two parts of water). The slips are then dried over the water bath. A drop of the urine suspected to contain sugar is allowed to fall on a small slip of the prepared merino, which is then dried, and exposed to the dull red heat of a spirit lamp. If a trace of sugar be present, a black spot is produced.

Bismuth Test.—Bottger has lately proposed a new test for sugar. This consists in adding first of all potash, then a small quantity of subnitrate of bismuth; lastly the mixture is boiled. If sugar be present, the oxide is reduced to metallic bismuth, which is precipitated in the form of a black powder. It has been asserted that sulphuret of bismuth is formed, but this seems not to be the case. Brücke shows that this test is more delicate than Trommer's (or the modification of it by Fehling); and he finds that the black precipitate is produced to some extent in specimens of healthy urine. The bismuth test may also be applied thus:—A solution of carbonate of soda (crystallised carbonate 1 part, water 3 parts) is prepared, and a certain quantity added to an equal amount of the urine. A little basic nitrate of bismuth is then added, and the mixture heated to the boiling point. If sugar be present, a black precipitate is produced. —*British Med. Journal, May* 19, 1860, *p.* 374.

50.—ON THE METHOD OF DETECTING AND ESTIMATING THE QUANTITY OF SUGAR IN DIABETIC URINE.

By Dr. James M'Ghie, Glasgow.

A considerable variety of methods have been given by different writers on urinary diseases, for testing diabetic urine, and estimating the quantity of sugar contained in any given specimen. The sense of taste is perhaps the readiest means of ascertaining that saccharine matter is present in urine, and evaporation gives an approximate estimation of the quantity. Trommer's test is now most commonly

employed, and the improvements made upon it by Barreswil and Fehling, enable us to employ it not merely for the qualitative, but also for the quantitative determination of sugar in the urine. The fermentation test, also, is one fitted to give satisfactory results.

Having been engaged in 1849 in the examination of diabetic urine, I had frequently tried all the tests usually employed, and among others that of Pittenkofer, with bile and sulphuric acid. At that time there was no notice of Runge's test in Dr. Golding Bird's work, though it appeared in a subsequent edition. Runge only contemplated the production of a blackish colour by applying dilute sulphuric acid to diabetic urine: while Pittenkofer employed it along with bile to produce a violet colour. Neither of these methods afford satisfactory results. "Grape sugar," says Thudichum, "is not influenced by dilute acids, and not blackened by concentrated sulphuric acid. If grape sugar dried at a temperature of 100°, is mixed with one and a half times its weight of concentrated sulphuric acid, it dissolves, and forms with the acid a combination which is an acid itself, and has been called sulpho-saccharic acid." However just this observation may be in reference to dried grape sugar, it does not apply to that form of sugar found in diabetic urine. When concentrated sulphuric acid is added to an equal volume of diabetic urine, and heat applied to ebullition of the mixture, the following changes occur: First, the fluid becomes blackish, and soon passes into a state of inky blackness. As soon as ebullition commences the mixture begins to froth up, and continues to be covered with a layer of froth so long as the boiling continues. This layer of froth also remains after the mixture is cooled. In no other kind of urine except the diabetic, are these appearances manifested. After the boiling has been continued for a short time, besides the intense black colour, when the fluid is closely examined there will be found a copious evolution of carbonaceous particles. The high specific gravity of the fluid in which these fine black particles are suspended, prevents their being deposited as a precipitate till it is largely diluted with water. In ordinary samples of diabetic urine a copious precipitate is speedily deposited on the addition of water. The precipitate should now be thoroughly washed on a weighed filter, till all the acid is removed, and nothing but the pure carbon remains on the filter. After being carefully dried and weighed, the excess of weight of the filter containing the carbon over the weight of the filter without the carbon, will give the amount of carbon contained in the sugar; and when this quantity is found it is easy to calculate the weight of the sugar. The dried precipitate on the filter will be found in the form of numerous small pieces of jet-black carbon, which may be collected, and, by combustion, will be dissipated in the form of gas, without leaving any solid residue.

It may be objected to this test, that there are other kinds of urine which, when boiled with sulphuric acid, develope appearances somewhat analogous, if not altogether identical with those produced by

diabetic urine. It is true that albuminous urine, when boiled with strong sulphuric acid, gives a deep rich brown colour, approaching to to black; but it presents none of the peculiar phenomena which we have noticed as accompanying diabetic urine when treated in a similar manner. The colour is different; it does not froth; and when diluted with water, there is no deposit of black carbonaceous matter, but the deep brown colour merely becomes less deep in shade.

Any probable fallacy as between albuminous and diabetic urine, will, if the following method is adopted, be completely dissipated. Let the urine be first acidulated with NO_5, and then boiled, when, if albumen is present, it will become coagulated, and may be removed by filtration, then proceed with the test as described above.

I have seen this test fail to develope the characteristic phenomena when applied after the urine has been kept a sufficient length of time to admit of decomposition taking place. A sample of urine which has already been found by Trommer's or some other test, to contain sugar, may, after it has stood some time, be tested by sulphuric acid, and will probably fail to produce any of the characteristic reactions. It will be found in all such cases that decomposition has begun, and not only this, but every other test will fail. The precaution ought to be taken to have the urine operated upon as fresh and recent as possible, when embarrassing failures of this kind will be avoided.

The *modus operandi* of the sulphuric acid in its action on grape sugar is extremely simple and well defined. The sugar consists of $C_{12}\ O_{12}\ H_{12}$; that is, twelve atoms of carbon are combined with twelve atoms of HO or water. Strong sulphuric acid has a sufficiently strong affinity for water to decompose the sugar, and remove from it all the elements of HO, and leave the insoluble carbon disseminated in fine particles through the mixture; and whatever action it may have upon the other constituents of the urine, it does not give rise to any other insoluble compound, if the necessary precautions have been taken at the outset, to remove the mucus by filtration, and the albumen, if there is any present, in the manner indicated above. In some specimens of high-coloured and concentrated urine, when there is a considerable quantity of animal extractive and colouring matter, the action of the sulphuric acid may develope a small amount of blackish particles, which, when treated in the same way as the carbon of diabetic sugar, are readily distinguished from it. It may be affirmed, without fear of contradiction, that there is no other kind of urine which comports itself in the presence of sulphuric acid in a manner analogous to diabetic urine. Grape sugar in the solid state is dissolved in SO_3, but in solution in the urine is decomposed by it. When treated with caustic potash and heat, the smell of burnt sugar is evolved, the solution becomes brown in colour, and glucic and molassic acids are produced. In the crystallized state it contains, besides the constitutional elements of water, two equivalents of water of crystallization, which are dispelled by a temperature of 212°. When the

temperature is raised to 284°, three equivalents of constitutional water are dispelled, and caramel is produced; but it is only when in a state of solution that sulphuric acid removes all the constitutional elements of HO. Of the ordinary tests Trommer's is preferred by some, Cappezuoli's and the fermentation test by others. The sulphuric acid test is one which, if deemed worthy of a trial, will, in point of simplicity in manipulation and certainty in its results, be found second to none.—*Glasgow Med. Journal, April* 1860, *p.* 42.

51.—ON THE DETERMINATION OF THE PROPORTION OF SOLIDS IN THE URINE OF HEALTH AND DISEASE.

By Dr. William Seller, F.R.S.E., Edinburgh.

Of late, my attention has been drawn to the modes of estimating the proportion of solid matter in the urine. There are still attendant on the reduction of the urine to dryness, great practical difficulties; and, respecting the proper rule for solving the problem by reference to the density of the urine, there are conflicting views among the best authorities.

I propose to offer a few observations bearing on each of these two methods; and first, of that which rests on the density.

It will be remembered that the three best-known proposals for finding the proportion of solids in the urine from the density are, to multiply the number denoting the excess of the density above 1000, 1st, by the number 2; 2dly, by a number less than 2 (1·65); 3dly, by a number greater than 2 (2·33).

This mode of proceeding rests on the circumstance, that when soluble matter is added to water, the solvent commonly, yet not always, undergoes an expansion.

If there be no expansion, there is no need of a multiplier—the excess of the density above 1000 signifies the amount of the soluble matter present. If the expansion expressed in water-grain measures be found to be equal to the excess of the density above 1000, represented by weight in grains, then the multiplier 2 will give the amount of soluble matter contained in the solution. For example, in the 1000 grain specific-gravity bottle, let 50 grains of soluble matter, the mean expansion caused by which is 25 water-grain measures, be dissolved in 975 grains of distilled water, the bottle will be exactly filled, and the specific gravity, which here corresponds with the actual weight, will be 1025. But 25, the excess of the density or specific gravity above 1000, multiplied by 2, gives the whole amount of solid matter, viz., 50 grains. Parallel to this is every case in which the expansion is equal to the excess of density above 1000. And thus, that form of the rule under consideration which makes 2 the multiplier, proceeds on the assumption that the number denoting, in water-grain measures, the mean expansion caused in water by the solid matter of

the urine is equal to half the amount of that matter expressed by weight in grains.

When, again, 1·65 is used as the multiplier, the method proceeds on the assumption that the expansion is to the excess of density above 1000, very nearly as 2 to 3, or is equal to two-fifths of the number denoting the weight of the soluble matter present.

Lastly, when the multiplier is made 2·33, the assumption exists that the expansion is to the excess of the density above 1000, nearly as 4 to 3, or is almost equal to four-sevenths of the number denoting the whole amount of solid matter present.

The whole of these statements is indisputable, provided it be admitted that a fixed weight of soluble matter of one kind creates the same amount of expansion during solution, whatever be the proportion of water. And though this proposition cannot be assumed as true without exception, yet it is certain that exceptions take place only to a small extent, chiefly when the point of saturation is approached.

It should be observed, as flowing from what has been affirmed above, that the number expressing the weight in grains of a salt present in solution, is made up of the excess of density above 1000, together with the number denoting the expansion in water-grain measures. Thus, when the specific-gravity bottle is filled with a solution weighing 1020 grains, the quantity of water present, if there has been any expansion at all, must be less than 1000 grains by the number of water-grain measures denoting the expansion; that is, the whole quantity, 1020 grains, is made up of the quantity of soluble matter, together with 1000 grains of water minus the number expressive of the expansion.

It should be remarked also, that, owing to the expansion being so uniformly constant for the same weight of a salt, whatever be the proportion of water, we reduce the specific gravity of a solution—for example, from 1050 to 1025—not by adding a volume of water equal to that which the solution is estimated to contain, but by adding a volume of water equal to that of the solution itself. Thus, 1000 grain-measures of urine of specific gravity 1030 may be found to contain no more than 970 grains of water; yet its specific gravity is reduced to 1015, not by adding other 970 grains of water, but by adding to it an equal volume, or 1000 grains, of water.

It should also be remarked, as flowing similarly from what has been affirmed above, that, for example, if four waters be poured upon a salt, and then four waters more, and so on in succession for a number of times, the solutions obtained will not present such a uniformly decreasing specific gravity,—that such a scale of decrease is obtained only when the successive quantities of water added are each equal to the first quantity, together with the expansion which took place while in the first instance the salt was taken up. Thus, if 250 grains of nitrate of potassa are dissolved in 1000 grains of water, the expansion amounts to nearly 90 water-grain measures; so that the quantities of water to

be added in succession to produce such a uniformly decreasing scale of density are, not 1000 grains, but 1090 grains each.

So much, then, for the principles on which the necessity for a multiplier rests.

We have next to consider the practical application of these principles to the case of the urine.

Of the practical rules before referred to, that most in favour in this country at present is the one proposed by Dr. Christison, not for the urine in general, but for the special highly dense urine of diabetes—that, namely, by which 2·33 is made the multiplier. While many adopt this rule implicitly, like Dr. G. Bird, who has founded two extensive tables upon it, others qualify it by the rule wherein the number 2 is made the multiplier, which they recommend when the density of the urine is lower in the scale, while they take Dr. Christison's rule only when the specimen of urine under examination is of high density. The third rule, that in which 1·65 is made the multiplier, was proposed by the eminent French observer, Becquerel. This last rule, however, has been, of late years, thrown entirely out of English books on the authority of Dr. Day, who, in an elaborate paper, has laid down Dr. Christison's rule as that most applicable to all kinds of cases.

The evidence on which each of these rules has been supported by their respective partisans, is the actual amount of solid matter obtained from numerous specimens of urine of known density. The objection to this evidence is the discrepancy in the results described by different observers,—which discrepancy is manifestly dependent on the difficulty of getting the solid residue perfectly free from water.

I am not aware that any one has hitherto attempted to try the merits of these several rules by the particular properties of the known constituents of the urine. This kind of inquiry, however, does not appear unworthy of attention. The plan of such an investigation I shall endeavour to suggest and follow out, as far as materials exist for the purpose.

The solid matter of the urine in health admits of a threefold division, namely, into—1, urea; 2, organic matters other than urea; and 3, inorganic saline substances. The proportions under these several heads, in many standard analyses of the urine in health, approach to an equality. Thus, in the analysis given by Miller in his *Chemistry*, the proportion of urea in 1000 parts of urine of specific gravity 1020, is 14·23; that of organic matters other than urea is 15·56; and that of inorganic saline substances is 13·35; while the proportion of water amounts to 956·80. Thus it may be allowed to us to assume, as a convenient diagram of the constitution of the urine, that the proportion in each of these divisions is equal, say 15 of each in the thousand parts. Such a diagram will stand thus:—Urea, 15; organic matters other than urea, 15; saline substances, 15; water, 1000, less the

amount of expansion: while the specific gravity will hardly exceed 1020. It is obvious that, were the medium expansion created in water by such a proportion of each of those divisions ascertained, a considerable step would be gained towards determining which of the multipliers before referred to should in general be preferred.

It is only in the case of the saline substances commonly believed to be present in the urine, that any evidence has hitherto existed with regard to the amount of expansion produced during their solution. When a substance can be obtained in a state of purity, nothing is easier than, within moderate limits of accuracy, to determine its property in this respect; for no more is necessary than to put a certain quantity of the substance into the thousand-grain specific-gravity bottle, and to fill it up with distilled water, then weigh the whole. It is manifest that the difference between the entire weight and the weight of the soluble substance put in is equal to the quantity of water present, and that the difference between the quantity of water and 1000 is the amount of expansion in water-grain measures. In this way I find that 30 grains of pure urea cause an expansion equal to 22 water-grain measures.

With respect to the organic substances other than urea, the small proportion of each, together with the difficulty of procuring most of them in a state of purity, has prevented me from making any direct observations as to their effect on the expansion of the solvent during solution. But it is evident that, in an exact analysis of the urine supplying the means of determining the effect of the urea and of the several inorganic saline substances on the expansion of the solvent, the means are furnished of inferring the expansion created by these organic substances in the aggregate.

Unfortunately, few recent analyses of the urine are of a kind to assist the mode of investigation here suggested; because it is not common, of late, to attempt to fix the exact proportions in which the several saline substances are present. The well-known analysis of Berzelius, now so time-honoured, is almost the only one I can find suitable to my present purpose. It is certain that no analysis has yet appeared fit to take the place which it has held so long. The only considerable defect in it is, that the specific gravity of the specimen of urine from which it was taken is not stated. It is less material that the organic substances other than urea and uric acid are not named in accordance with present views.

The proportion of solids to the water in this analysis is remarkably high, being 67 to 933 on 1000 parts by weight, or, in round numbers, 70 grains in 1000 water-grain measures of urine. If the specific gravity be sought by the converse of the rule so often referred to—that is, by dividing the quantity of solids present in 1000 water-grain measures by one of the three numbers, 1·65, 2, or 3·33—the answers will be respectively 1043, 1035, and 1030. The first number, 1043, is certainly considerably above any specific gravity which we are accus-

tomed to meet with during health in this country. Nevertheless the case may be different in Sweden. As to 1030 and 1035, they are by no means uncommon as characterizing the urine of particular parts of the day, chiefly when the urine is very slowly secreted. The chief peculiarity, besides, in this analysis is the large proportion of urea. It has, however, been remarked, that the slow secretion of the urine with a small proportion of water is the circumstance most favourable to the elimination of an unusually large proportion of urea. And it is probably this same circumstance which has given rise to the prevailing idea, that it is the large proportion of urea which principally contributes to a high specific gravity of the urine. The truth, however, is, that when the urine has a high specific gravity, in health, there is an abundance of urea; but it is not the abundance of urea which chiefly contributes to that high density, for even 70 grains of urea, contained in 1000 grain measures of its simple solution, give rise to a density no higher than 1019. There is thus no reason to doubt that this analysis correctly represents a genuine specimen of healthy urine.

First, then, of the inorganic saline substances in this analysis. These salts are the sulphate of potassa, the sulphate of soda, the phosphate of soda, the phosphate of ammonia, the chloride of sodium, the hydrochlorate of ammonia, earthy matters with a trace of fluoride of calcium and siliceous earth. The two last particulars being omitted, the sum of these salts in the analysis is 17·41, and the sum of the expansion which they create in water during their solution, calculated from data obtained, for the most part, from authentic chemical sources, is 7·26 water-grain measures. Thus, a solution of these salts in 992·74 grains of water would yield 1000 water-grain measures of a fluid having a specific gravity of 1009·15. If this fluid were a true type of the urine, the two lower multipliers would give a more correct indication of the amount of solids present than the highest; for 9·15 multiplied into 2·33 give 21·31—an excess of nearly 4 grains above the quantity of salts present, viz., 17·41.

But urea, which in the analysis under consideration is present in large proportion, produces a very different effect on the urine from these salts. It was stated above, that 30 grains of urea create an expansion during solution equal to 22 water-grain measures. Thirty grains of urea being the quantity stated in the analysis in 1000 grains of urine, there is more than 30 grains in 1000 water-grain measures of urine; and, therefore, the expansion due to it is correspondingly greater than 22 water-grain measures; but that small difference may be left out of view. If, then, the fixed salts (17·45 grains) and the urea (30 grains), together 47·45 grains, are dissolved in water, the sum of their expansion is (22 + 7·26) 29·26; so that 970·74 grains of water will suffice to produce a solution 1000 water-grain measures in volume. The specific gravity of this solution is 1018·19; and even the highest multiplier, 2·33 fails to bring out so

large a quantity as the solids present are equal to; for 2·33 × 18·19 are only equal to 42·38, the multiplier required being 2·6, since 18·19 × 2·6 = 47·29.

We have next to consider the effect of the organic matters other than urea. Here the uncertainty of the specific gravity creates a great difficulty. Thus, were we sure that the specific gravity is 1030, the aggregate of the expansion produced by the three divisions of the solids would be known, since it is the whole amount of the solids diminished by the excess of the density above 1000; or it is 70 — 30 = 40. Hence it would at once appear that the expansion created by 18·46 organic substances other than urea amounts to 10·74, or the difference between 40 and the expansion already seen to belong to the sum of the urea and the inorganic salts, namely, 29·26.

Since, however, this uncertainty as to the specific gravity of the specimen of urine under consideration prevents any trustworthy inference as to the effect of the organic matters of the urine other than urea in the expansion of the solvent, recourse must be had to some other analysis in quest of such information.

In Miller's analysis, the amount of fixed salts, 13·35 gives rise to an expansion of no more than one-third, $\frac{13·35}{3}$, or 4·45, while 14·23 parts of urea create an expansion of 10·43,—the joint expansion, therefore, effected by the fixed salts and the urea is 14·88. The whole quantity of solid matter in this specimen of urine is 43·2, and the specific gravity is 1020. The total expansion is discovered by taking the excess of the density above 1000, namely 20, from the whole quantity of the solids, 43·2, when there remains 23·20. But this last number being diminished by that denoting the joint expansion produced by the fixed salts and the urea, namely 14·88, there remain 8·32 as indicating the expansion attributable to the organic substances other than urea, which amount to 15·56. But 15·56 : 8·32 :: 18·46 : 9·86. Thus there is deduced from Miller's analysis a number, 9·86, expressive of the expansion due to the 18·46 of organic substances other than urea contained in the analysis of Berzelius,—a number differing in a very slight degree from that, namely, 10·74, inferred from the assumption of 1030 as the specific gravity of that specimen of urine.

We may regard it as proved, then, that the true specific gravity of the specimen of urine to which the analysis of Berzelius applies is 1030. And it should be observed, also, that the highest multiplier, namely, 2·33, gives a nearly exact result as to the whole quantity of solids present; for 30 × 2·33 = 69·90, or, as nearly as possible, 70, —the round number assumed above as denoting the quantity of solids present in 1000 water-grain measures of this specimen of urine.

It must be confessed, however, that little progress can be made towards determining by this method which multiplier deserves the preference, until many trustworthy analyses of the urine, as it exists

under different circumstances compatible with health, can be procured on the exact plan adopted by Berzelius. In the meantime, some progress might be made, at no great cost of trouble, if those who have leisure were to accumulate information by determining the exact proportion of urea and of inorganic salts in a number of specimens of urine. It is easy to determine the proportion of urea by the method of Liebig, to which I endeavoured to draw attention in a recent number of this Journal; and it is a work of no great difficulty, to those who have time on their hands, to determine the amount of inorganic salts. Thus, if the urine be reduced by heat to the consistence of syrup, and pure nitric acid be added to the extent of 4 or 5 per cent. on the original quantity of the urine; if the evaporation be continued till the whole will solidify on cooling; and, lastly, if portions of the mass be projected into a red-hot platinum capsule, the fixed salts will be obtained in the form of a white mass. It is true that, after we have ascertained the proportions of urea and of the fixed salts, we cannot discover the proportion of the organic matters other than urea unless we know the whole amount of solid matter present. But of an easy method for this purpose I am going to speak before closing this paper.

Before leaving this subject I will only add a single illustration, by way of showing the point from which any further investigation must set out. Let us recur, then, to the diagram of the constitution of the urine in which the solid matter was regarded as falling under three divisions, in each of which the proportion was held to be 15, while the amount of the solvent was taken at 1000, less the total expansion. Of the amount of expansion produced by these several divisions, there is no certainty except in the case of that consisting of urea. With respect to the expansion produced by the other two divisions, though a figure can be produced by calculation for each, yet, owing to the insufficiency of the data, these numbers must be regarded as merely provisional. For every 15 parts of urea dissolved, it is certain that the expansion is nearly 11; for every 15 parts of organic substances other than urea, the provisional number denoting the expansion is 9; and for every 15 parts of inorganic salts, the provisional number denoting the expansion is 5.

Let us first attend to the result when the formula is used without change on these numbers. If 15 parts of each of the three divisions of constituents be together dissolved in 975 parts of water, the aggregate of the expansions is 25, so that the solution occupies 1000 water-grain measures. The total amount of solid matter is 45, from which the specific gravity may be deduced by subtracting from it the number denoting the entire expansion, $45 - 25 = 20$, which is the excess of the density above 1000. Or the specific gravity in this case is discovered by estimating the total weight of the solution, thus:—Water, 975; solids, 45—sum of both, 1020. Here the excess of the density above 1000 is 20; which number, with the

highest multiplier, gives a product somewhat in excess of the amount of the solid matter present, $20 \times 2{\cdot}33 = 46{\cdot}60$.

If 6 parts of urea and 15 parts of each of the other two divisions be dissolved, the aggregate of the expansions, viz., 4·3, 9, and 5, amounts to 18·3; which number, taken from the entire sum of the solid matter, namely, 36, gives 17·7 for the specific gravity. Here, then, the number 2, as multiplier, gives very nearly the actual quantity of solid matter present. Again, if 34 parts of inorganic salts, 8 parts of urea, and 4 parts of organic matter other than urea, be dissolved, the total expansion is nearly 18 (nearly 11, 4·7, and 3), which, taken from the whole amount, 46, leaves 28 for the specific gravity. But 28 gives, with the least multiplier, 1·65, very nearly the exact amount of solids present, viz., 46·48. If, then, the above diagram of the general constitution of the urine be found to come near the truth, and if the numbers adopted be discovered, on further investigation, to express nearly the average expansion created by the organic matters other than urea and the inorganic salts, the highest multiplier, 2·33, proposed by Dr. Christison and extended by Dr. Day, is that which decidedly deserves the preference.

The conclusion to which many will be disposed to come, from what has been advanced in this paper, will probably be, that the settlement of the point at issue is not of sufficient utility to deserve so much trouble. It may be so; but the inquiry itself, independently of the result, cannot but lead to the accumulation of useful details on a subject of growing importance in practice.

For example, I do not find that attention has been anywhere drawn to the remarkable effect of urea in creating expansion of the solvent during its solution. The sugar of grapes, commonly regarded as identical with the sugar of diabetes, has, I find, a similar property —the sugar of grapes, dissolved to the extent of 50 grains, gives an expansion equal to 30 water-grain measures. Cane-sugar has nearly the same effect.

With regard to the mode of determining the specific gravity, it appears to me that an urinometer with a properly constituted scale, the requisite attention being given to variations of temperature, is sufficient for all purposes connected with practical medicine. It should be seen to that the scale is properly graduated. I observe a very singular reason, given most unaccountably both by Golding Bird and Thudichum, for the inequality of the degrees of the scale, namely, that "the density of each stratum of fluid in which the instrument is immersed, of necessity increases with the depth from the surface." The degrees of the scale are necessarily unequal, because each is the sign of a new proportion; since the volume of the portion of the instrument immersed is in the inverse ratio of the density of the fluid into which it has been plunged. If any further reason be required, I should venture to say that the higher the urinometer stands out of the fluid, the greater is the pressure,

owing to the difference of weight in water and in air, with which it resists the buoyancy of the part immersed, so as to render the degrees shorter in proportion as these approach the lowest part of the stem.

I lately extemporised an urinometer, on the plan of which a convenient instrument might, I think, be made. I took a small pipette, and, having closed the lower aperture by the gas flame, I put a sufficient quantity of mercury into the interior to cause the instrument to sink perpendicularly below the level of the bulb. The pipette now floated, in pure water, perpendicularly with the water-line, nearly midway between the bulb and the upper extremity. I then fixed a thin slice of a common cork on the upper end, so as to form a small table on which weights might be laid. I also passed a wire with a slender point perpendicularly through the cork till it touched the surface of the water, to serve for an exact index to the height at which the instrument stood in the water. When this rude instrument was placed in a fluid denser than water, it rose, of course, throwing even a portion of the bulb above water; but, by placing weights on the little cork table, it was made to sink to the level at which the point of the wire grazed the surface of the water. Since by this means the same bulk of fluid is evidently always displaced, whatever be the density and whatever the temperature at the time, provided the weights on the little cork table are adjusted to keeping the point of the perpendicular wire just grazing the surface of the fluid, those weights will exactly indicate the difference between a given bulk of distilled water and the same bulk of any denser fluid. Whatever be the temperature at the time, the index for distilled water is shifted to answer for that temperature. If the bulk of fluid displaced by such an apparatus be not known, all that is requisite, for example, in the case of the urine, is to immerse the instrument in a fluid known to have a density of 1060, and to ascertain how many grains suffice to sink the point of the wire to the surface; and the product of this number divided by 60 will answer to the weight required for each degree between 1000 and 1060. Here there is no inequality of degrees. The sixtieth part of the difference between the weight of the volume of distilled water displaced, and the same volume of the standard fluid at 1060 equally displaced, corresponds to a degree. I find it convenient to use pieces of thin lead-wire in such experiments, owing to the facility with which these can be adjusted to any weight. Such an instrument, of course, is better suited to the study than to the sick-chamber.

With respect to the determination of the proportion of solid matter in the urine by evaporation to dryness, it has long been manifest that the difficulty of the ordinary methods place them entirely beyond the reach of medical men for practical use. A mode, however, occurred to me some time ago, which I have repeatedly put to trial. This mode is, I think, susceptible of very great accuracy, and at very

little cost of time. It must be confessed, nevertheless, that without due precautions, it may lead into very considerable errors. It is founded on so simple an observation as that common filtering paper readily takes up a large proportion of urine, and, under favourable circumstances, dries in no long time. The only considerable source of error is the difficulty of bringing back the paper to exactly the same hygrometric state in which it was at the beginning of the experiment. The paper is to be carefully weighed before being moistened, and weighed again with the same care after it has become dry—the difference in weight is the amount of the solid matter in the quantity of urine employed. For 1000 grains of urine, or rather for 1000 water-grain measures of urine, I have commonly used three sheets of filtering paper, cut each into four pieces. The urine is poured into a well-dried basin, and each piece is moistened in succession, as far as it will bear without dripping; enough being left unwetted at its upper part to allow of a fold, by which it may be hung on a string or wire to dry. For this effect a few hours suffice. If the temperature and hygrometric state of the apartment can be kept uniform during the whole process, there is plainly no difficulty; if the drying be carried too far, there is an error of deficiency in the amount of the solids inferred; if the drying be insufficient, or if the apartment has declined in temperature, or its hygrometric state has become greater, there is an error of excess. It should be remembered that the paper, though apparently dry, readily absorbs moisture, owing not only to its original tendency, but to the deliquescent character of the solid matters contained in the urine.—*Edinburgh Med. Journal, Aug.* 1860, *p.* 105.

52.—ON THE FREQUENT OCCURRENCE OF PHOSPHATE OF LIME, IN THE CRYSTALLINE FORM, IN HUMAN URINE, AND ON ITS PATHOLOGICAL IMPORTANCE.

By Dr. Arthur Hill Hassall, London.

Read before the Royal Society. (Communicated by Dr. Sharpey, Sec. R.S.)

In 1854 I submitted to the Royal Society a paper, 'On the Frequent Occurrence of Indigo in Human Urine.' The communication, which was published in the 'Philosophical Transactions,' attracted considerable attention both at home and abroad. The singular fact of the frequent preeence of indigo in the urine, first announced by me, has since been amply confirmed by a variety of observers. I have now to place before the Society some investigations in relation to the not uncommon occurrence in human urine of *phosphate of lime*, as a *deposit*, in a well-marked *crystalline* form.

When the earthy phosphates are treated of by writers, in connexion with the urine, they are usually described collectively, and it is sel-

dom that each kind of phosphate is particularized, and yet there are several which may occur either separately or together. The phosphate of ammonia and magnesia, or triple phosphate, is often specified, but rarely is phosphate of lime separately mentioned, and phosphate of magnesia scarcely ever; and yet phosphate of lime is very frequently present as a deposit in urine, much more so, indeed, according to my experience, than the triple phosphate, excluding those cases of the occurrence of that ammoniacal phosphate arising from the decomposition of the urea of the urine subsequent to its escape from the kidneys. Even in those few cases in which phosphate of lime is specially mentioned, it is described *usually* as mixed up with the other phosphates, and *always* as occurring in the *amorphous or granular*, and never in the crystalline state; further, no peculiar importance is attached to it, as contrasted with the magnesian phosphate.

Even one of the most recent writers on the urine gives the following description of the physical characters of deposits of phosphate of lime in urine:—"Deposits of phosphate of lime," he states, "as usually occurring in the urine, and mixed with magnesia, are always white and amorphous, under the microscope appearing in granules, sometimes of a greenish tinge, which exert a refracting action upon light. *Crystallized deposits of this substance have not been observed.*"

I now propose to show, first, that there is a crystalline deposit, the crystals composing which will be described hereafter, which does really consist of phosphate of lime; second, that it is of frequent occurrence in human urine; and third, that it is of greater pathological importance than the deposits of triple phosphate.

I would first remark that I have for years been acquainted with the fact of the occurrence of crystalline phosphate of lime in the urine, and I have referred to it in the Lancet of 1853, and also elsewhere. I should now remark, however, that the statement made by me as to the composition of the crystals, has hitherto been based upon their *qualitative* analysis only, and therefore was not so completely conclusive and satisfactory as could be desired. Until recently I had not made any *quantitative* analyses: these I have since been enabled to perform, and I now furnish the results of the chemical examination of four samples of the deposit.

First Sample.—Filtered from the urine of twenty-four hours; mixed, as ascertained in the first instance by means of the microscope, with a very minute quantity of *triple phosphate.*

Bibasic phosphate of magnesia	0·15
Bibasic phosphate of lime	1·85
	2·00

Second Sample.—Filtered from urine after the lapse of a day or two; mixed with a small quantity of *triple phosphate.*

Bibasic phosphate of magnesia	0·47
Bibasic phosphate of lime	6·18
	6·65

Third Sample.—From urine of twenty-four hours, after the lapse of several days. Admixed with much triple phosphate, as shown first by the microscope, and afterwards by the chemical analysis.

Bibasic phosphate of magnesia	4·30
Bibasic phosphate of lime	5·41
	9·71

Fourth Sample.—Separated from six ounces of fresh urine. Deposit very pure.

Bibasic phosphate of lime	1·96
No phosphate of magnesia.	

Now the admixture of the phosphate of magnesia in the first three samples was due solely to the fact, that the phosphate of lime deposited at first in the pure state, was allowed to remain in the urine until decomposition had commenced, and the phosphate of magnesia and ammonia had, in consequence, become formed. Deposits of phosphate of lime are sometimes contaminated from the same cause with carbonate and oxalate of lime.

These analyses are therefore conclusive as to the composition of this earthy phosphate. In order to show that no error has been committed in them, I here append the process adopted. That the deposit in question really consisted of a *phosphate*, was first repeatedly determined by the action of a solution of nitrate of silver; the crystals, when touched with this reagent, assumed a bright golden-yellow colour. After having been separated and washed in distilled water, the phosphate was ignited to free it from animal matter, urea, &c., and weighed. It was then dissolved in hydrochloric acid; ammonia was added until a permanent precipitate formed; this was re-dissolved by the addition of acetic acid. First the *lime* was precipitated from the solution by oxalate of ammonia, and afterwards the *magnesia* as follows:—Chloride of ammonium was added, then ammonia in slight excess, and, lastly, phosphate of soda. The oxalate of lime formed was converted into carbonate of lime in the ordinary manner, and the phosphate of ammonia and magnesia into the pyro-phosphate of magnesia; these were then weighed separately, and the amounts of the bibasic phosphate of lime were determined by the usual calculations. The results obtained corresponded very closely with the original weights of the ignited phosphates subjected to analysis. The analyses,

therefore, show that the crystallized phosphate of lime is a tribasic phosphate, containing two atoms of lime, and, most probably, one of water.

Form of the Crystals.—The size, form, and arrangement of the crystals of phosphate of lime, as they occur in human urine, vary greatly, but the peculiarities are in all cases sufficiently characteristic to allow of the ready identification of this phosphate by means of the microscope. The crystals are either single or aggregated, most frequently the latter, forming glomeruli, or rosettes, more or less perfect. Sometimes they are small and needle-like, and then they frequently form, by their crossing and union at right angles, glomeruli or spherules. Sometimes the crystals are thin and flat, having oblique or pointed terminations. Very frequently, however, they are thick, and more or less wedged-shaped, and united by their narrow extremities so as to form more or less complete portions of a circle; the free larger ends of the crystals are usually somewhat oblique, and the more perfect crystals present a six-sided facette. I have never yet met with these crystals having both ends perfect, owing, I believe, to the tendency which they have to crystallize from a centre in rosettes. When these crystals are kept in the dry state for a long time, they not unfrequently break down and crumble into powder.

The late Dr. Golding Bird, in his work on "Urinary Deposits," has given a representation of some crystals which he has denominated "*penniform*," describing them as consisting of a variety of the magnesian phosphate; the crystals figured do, however, undoubtedly represent a modification of those of *phosphate of lime*. Although I have elsewhere pointed out this error, most recent writers on the urine still persist in describing these crystals as a variety of the phosphate of ammonia and magnesia.

On the frequency of their occurrence.—I find, as already stated, that phosphate of lime, in the form of crystals, is of much more frequent occurrence in human urine than the triple phosphate, excluding those cases of the presence of the latter phosphate which are due to the decomposition of the urea of the urine subsequent to its emission. I have met with deposits of crystallized phosphate of lime in some hundreds of urines, and in many different cases; it is therefore not a little remarkable, from the frequency of its occurrence, and the peculiarities presented by the crystals, that it should have been so long overlooked. The microscope, therefore, furnishes us, in most cases, with the ready means of detecting the presence of deposits of phosphate of lime, as of so many other urinary deposits.

Characters of Urine depositing Crystallized Phosphate of Lime.—The urine from which phosphate of lime is deposited is usually pale, but occasionally it is high-coloured; the quantity passed is large, and the calls to void it frequent, more or less uneasiness and smarting being occasioned by its passage, at the neck of the bladder, and along

the course of the urethra; its specific gravity varies greatly. Taking the whole quantity passed in twenty-four hours, it is usually below the average, nevertheless the animal matter and urea are absolutely in excess. It is generally feebly acid, often decidedly so when first voided, the greater part of the phosphate of lime becoming deposited while the urine still retains some degree of acidity; it, however, speedily becomes alkaline, owing probably to the excess of mucus contained in it. Sometimes the crystals of phosphate of lime are thrown down from the urine before its escape from the bladder; ordinarily, however, the urine is bright and clear when passed, and the crystals are not formed until some time after it has been voided. In collecting this phosphate for analysis, the object being to procure it in as pure a state as possible, and as free from phosphate of ammonia and magnesia, oxalate and carbonate of lime, it should be separated from the urine very soon after it has become deposited, and before decomposition has had time to set in.

On the Pathological Importance of Deposits of Phosphate of Lime in Human Urine.—Of the pathological importance of excess of phosphate of lime in the urine not a doubt can be entertained, but certain reasons and facts may be advanced to show that deposits of that phosphate have a deeper pathological significance than those of the phosphate of ammonia and magnesia. The proof of this is the more necessary, since writers on the urine are in the habit of describing, as well as of treating, deposits of the earthy phosphates collectively, and without distinguishing between them: this course was natural enough so long as they were unacquainted with the fact that deposits of phosphate of lime, in the state of crystals, are of frequent occurrence, or so long as they mistook them for a variety of the ammonio-magnesian phosphate. One reason why we should be disposed to attach greater importance to the excess of the calcareous than the magnesian phosphate, is that most of the phosphoric acid of this last phosphate, and all the magnesia, are derived from *without*, being contained in the various articles consumed as food; while for the phosphate of lime, we have in the system—in the teeth and bones, and also in the nitrogenous tissues—sources containing some pounds weight of this phosphate.

That the osseous system is subject to disintegration is certain, and that the extent and rapidity of this differ remarkably in different cases is equally so. This is shown by the simple fact alone, of the early and rapid decay of the teeth in many persons. For this general reason therefore only, we should, *à priori*, be disposed to attach greater importance to the occurrence of deposits of phosphate of lime than those of phosphate of magnesia.

Other facts tending to confirm this view are,—first, that while deposits of phosphate of lime are frequently met with, those of phosphate of magnesia (not the ammonio-magnesian phosphate) are ex-

ceedingly rare; and second, that the calcareous is of more difficult solubility than the magnesian phosphate. This last circumstance explains probably why phosphate of lime falls as a deposit from acid urine, while phosphate of magnesia remains in solution.

The particular or special reasons for regarding deposits of phosphate of lime as of more moment than those of the triple phosphate, are derived from direct pathological observation. I have observed that when this deposit occurs, it is very apt to be persistent; and when it has disappeared, to return whenever the health is reduced from any cause. I have also noticed, that when it is persistent, it is usually associated with marked impairment of the health, and this often where organic disease does not exist. The prominent symptoms in one case of calcareous phosphatic deposit which I have had under observation for some years, were,—great disorder of the digestive organs, frequent and distressing headaches, occasional vomiting, debility, emaciation, great irritability of the nervous system, sexual powers weak, pulse slow and feeble, skin cold, urine in excess, of rather low specific gravity, acid when passed, but soon becoming alkaline, micturition frequent, with irritation at neck of bladder and in the course of the urethra; teeth much decayed. It should be stated that there is in this case a very slight tendency to paralysis of the right leg, as shown by an occasional sensation of coldness in the limb, and slight deficiency of power in it at times only. This symptom is, however, by no means a constant or necessary one in such cases.

If these views of the pathology of phosphate of lime be correct, we should expect to find an excess of that phosphate in the urine in great and rapid waste of tissue, during the rapid decay of the teeth, and in cases of mollities ossium. That there is an excess of the calcareous phosphate in the urine in these cases, is shown alike by observation and analysis.

It is obvious from this imperfect sketch, that much remains to be effected in regard to the pathology of phosphate of lime; but now that the frequency of its occurrence in human urine as a crystallized deposit is made known, its pathology, apart from that of the triple phosphate, will no doubt be specially considered.

It will be apparent from the following quotation, that the late Dr. Golding Bird regarded deposits of phosphate of lime as of more consequence than those of the triple phosphate:—"The pathological state of the system accompanying the appearance of deposits of phosphate of lime is analogous to that occurring with the triple phosphate; indeed, as has been already observed, they often, and in alkaline urine always, occur simultaneously. So far as my own experience has extended, when the deposit has consisted chiefly of the calcareous salt, the patients have appeared to present more marked evidence of exhaustion, and of the previous existence of some drain on the nervous system, than when the triple salt alone existed, unless its source is strictly local."

It should be remembered that these remarks of Dr. Bird refer to deposits of phosphate of lime in the *granular state*, and not to the crystalline deposits, with the occurrence of which he was unacquainted. I have already stated that, according to my experience, the granular calcareous phosphatic deposits are much more rare than the crystalline.

It follows from these observations and investigations :—

1st. That deposits of *crystallized* phosphate of lime are of frequent occurrence in human urine, much more so, indeed, than those of the amorphous or granular form of that phosphate.

2nd. That the crystals present well-marked and highly characteristic forms, whereby the identification of this phosphate, by means of the microscope, is rendered easy and certain.

3rd. That there is good reason to believe that deposits of phosphate of lime are of greater pathological importance than those of the phosphate of ammonia and magnesia.—*Lancet, Aug.* 4, 1860, *p.* 111.

SURGERY.

AFFECTIONS OF THE BONES AND JOINTS, ETC.

53.—CASES OF EXCISION OF THE KNEE-JOINT.

[The following nine cases appear in the 'Lancet,' in its "Mirror of the Practice of Medicine and Surgery in the Hospitals of London." Probably statistics would now show a very fair amount of success in cases of excision of the knee-joint. The operation is undertaken with more confidence than formerly, and, in suitable cases, is gradually superseding amputation. The editor observes:]

To speak of particular points in connexion with excision of the knee, it seems to be questionable whether the operation should be resorted to in the young, because the affected limb, after the loss of its main joint, does not grow proportionately with other parts of the body. This has been found to be true in some cases; nevertheless, the shortened limb with a high-heeled shoe is preferred by the patients themselves to going about with a single leg. It would appear that there is scarcely a disease of the articulation, excluding of course anything in the shape of malignancy, for which excision may not be practised. It may be done for even extensive necrosis; indeed we can call to mind other instances besides the subjoined wherein Mr. Fergusson adopted this mode of treatment with decided advantage; and we have the recent experiments of M. Ollier, of Lyons, to prove that if the periosteum be left entire in the vicinity of joints, it will reproduce bone.

At one time the patella was allowed to remain, because it added support and strength to the knee; but subsequent experience has tended to show the advantage of its removal. Its retention is still a subject for consideration.

The great inducement to perform excision of the knee-joint at the present day is the fact that the per-centage of deaths consequent on this operation is much less than that attending amputation; a reference to the memoirs of Mr. Butcher and Mr. Price will satisfactorily prove the truth of this. At Guy's Hospital, Mr. Bryant has shown, the per-centage of deaths in *pathological* amputations is 1 in 5·5, the same as in excision of the knee; whilst that in *chronic disease* is 1 in 7, being somewhat more favourable. The statistics of amputations elsewhere, taken generally, will not give such favourable results as these.

Removal of Necrosed Bone, with subsequent Excision of the Knee-Joint, in a lad aged 14: Recovery. Under the care of PROF. FERGUSSON, at King's College Hospital.

E. H., aged fourteen, was admitted Dec. 27th, 1859. He stated that in May, 1858, he was being carried down some steps, when his knee came in contact with the iron railings; the joint was slightly painful at the time, but he thought little of it, and it improved by fomentations and rest. About six weeks after the accident, however, he experienced a good deal of sharp, shooting pain in the joint, with considerable swelling. He sought medical advice, but no remedies appeared to have a beneficial effect upon it. This state of things continued three months, when an abscess was discovered to exist in the joint, which was opened, and an immense quantity of matter evacuated. This gave him some temporary relief, but the knee had now become considerably flexed, obliging him to adopt crutches to enable him to get about. He continued much in this state up to the time of admission.

The patient is a pale, delicate, strumous-looking boy. He lies on his left side: the thigh being flexed on the trunk, the leg on the thigh. There are several sinuses in the region of the joint leading to bone; one, on the inner and lower part of the thigh leads to an extensive bare surface of the femur. The patient has no brothers or sisters; father and mother healthy. No lung affection; other functions of the body natural.

Jan. 7. The patient having been narcotized, Mr. Fergusson proceeded to remove a piece of the shaft of the femur about six inches in length, by an incision of two inches, made at the lower end of the thigh. The removed portion was easily detached, and found to be necrosed.

8th. Has had a good night; complains of slight thirst. Limb rather uncomfortable; pulse 106.

10th. Wound suppurating healthily; the patient says the limb is not so painful as before the operation; appetite good.

Feb. 2nd. Has gone on well since last report; eats, drinks, and sleeps well; is anxious to have his leg put straight.

March 3rd. To day the operation of excision of the knee-joint was performed by Mr. Fergusson, the patient being under the influence of chloroform. The H incision having been made in the integuments, sufficient of the ends of the femur and tibia was cut off to bring the limb in the straight position. The patella was removed. There was very little hemorrhage during the operation, no vessel requiring a ligature. A splint having been applied, the patient was sent to bed.

4th. Has had a restless night, the limb having been very painful; but the pain has been much mitigated by an opiate draught; pulse 120.

5th. Is much better to-day in health, but the leg continues to give great pain at intervals. To have three grains of sesqui-carbonate of ammonia in an ounce of decoction of cinchona every four hours.

10th. Wound discharging healthily; no bad symptom; appetite good; bowels regular; all pain has disappeared.

20th. Is still going on well, the wound discharging a thin, serous fluid, mixed with pus; splint removed; the joint seems firm; splint reapplied.

May 1st. Gutta-percha splint applied to the back of the leg.

9th. Discharged to go into the country.

Six Cases of Excision of the Knee-Joint performed at St. Thomas's Hospital during 1859*: Recovery in Five.* Under the care of Mr. SOUTH, Mr. SOLLY, Mr. LE GROS CLARK, and Mr. SIMON.

The operation in the following cases was performed much in the same manner. Chloroform was given, and the joint was opened by an elliptical incision, which included the patella in the flap. The ends of the bones and the joint surface of the patella were removed by Butcher's saw, and the limb was put up in a slightly-bent iron back splint. Liberal diet and stimulants were given, and all the patients had cod-liver oil and some preparation of iron.

Case 1.—A strumous, delicate man, aged thirty-seven; had disease of the sternum and ulceration of the cartilages of the left knee-joint. He suffered much pain, his rest was disturbed, appetite bad, and his health generally broken. Two sinuses, connected with the joint, discharged a watery pus. Excision was performed by Mr. South, Nov. 5th, 1859. The cartilages were found extensively eroded, and the head of the tibia secondarily affected to a slight extent. At first, after the operation, his health improved; the wound united in part, and healthy suppuration was established: but towards the end of February diarrhœa of a very obstinate character set in, with night-sweats. By the end of March his condition was by no means satisfactory; there was no apparent union of the bones, his diarrhœa continued despite the use of various astringents, he was sick at times, and his appetite was capricious. The wound did not discharge excessively, and it looked healthy. From this time there was but little alteration in his condition, and certainly none for the better, and he went out, at his own particular request, on the 2nd of June. There was then no amount of bony union, the wound was discharging a tolerably healthy pus, and he did not suffer pain. He died a short time after leaving the hospital.

Case 2.—A very strumous-looking boy, aged six, with chronic ulceration of the cartilages of the right knee-joint, the tibia being dislocated backwards. He was hectic; suffered much pain; and there was a discharging sinus connected with the joint. Excision was per-

formed by Mr. Solly on the 9th of November, 1859. The cartilages were extensively eroded and the ligaments destroyed. After the operation he improved at once, never having a bad symptom. The wound healed; he gained health and strength, and left the hospital with scarcely any perceptible shortening of the limb, being able to walk with ease and comfort.

Case 3.—A delicate, scrofulous boy, aged eight, with chronic ulceration of the cartilages of the knee-joint; free discharge from three sinuses. He was hectic, and suffered much pain. Excision was performed on the 5th of March, 1859, by Mr. Solly. There was a considerable erosion of the cartilages, and false membrane in large quantities; but no disease of bone. The wound united in great part by first intention; his health improved; he slept well; and his appetite was excellent. He left the hospital quite stout and well; the limb was only half an inch shorter than its fellow, and he could walk with ease and rapidity.

Case 4.—A strumous boy, aged twelve, with long-standing suppuration in the knee-joint and several discharging sinuses. He was much out of health, and suffered greatly. Excision was performed by Mr. Le Gros Clark on the 12th of March, 1859. The cartilages were found to be in great part removed, and there was some secondary disease of the head of the tibia. A considerable portion of the wound united by primary intention. He slept well, and took his food with relish. During the progress of the case he had a slight attack of erysipelas, and an abscess formed over the ankle. Despite these drawbacks, the result of the case was favourable; and when he left the hospital there was firm bony union, and only a small portion of the wound uncicatrized. The limb was but slightly shortened.

Case 5.—An emaciated, sickly girl, aged six, with scrofulous disease of the knee-joint. She had suffered much; slept badly; and had but little appetite. Excision performed Nov. 12th, 1859, by Mr. Simon. The cartilages were eroded, and there was softening of the end of the femur. For a few days after the operation her condition was very critical, but when suppuration was established she improved, slept better, and was more cheerful. She was sent from the hospital to the Margate Infirmary. There was firm bony union, and only a slight amount of ulceration in the line of the incision when she left St. Thomas's.

Case 6.—An unhealthy, pasty-looking man, aged twenty-three, with long-standing disease of the knee-joint. After his admission two incisions were made, and considerable quantities of pus escaped. He did not improve much, and excision was performed Nov. 19th, 1859, by Mr. Simon. There was extensive ulceration of the cartilages, and superficial caries, dependent upon a small sequestrum in the head of the tibia. He improved in health after the operation, and a good deal of the wound united by first intention. A counter opening was required, when suppuration occurred, to give exit to some retained

pus. When he left the hospital there was firm union of the bones, he was able to get about, and was in very fair health.

Two Cases of Excision of the Knee-joint performed at the Royal Free Hospital; Recovery. Under the care of Mr. GANT.

Case 1.—John P., aged eighteen, was admitted on Oct. 19th, 1859. The patient is a strumous, unhealthy-looking subject; a coal-porter. While following his occupation about four years and a half ago, a large piece of coal fell on his right knee, and occasioned pain for some days, after which he resumed his work, although the joint was much swollen, and still painful. He continued to work during the ensuing three months, the joint remaining swollen, and latterly the hamstring muscles contracted so as to draw the limb backwards to nearly a right angle with the femur. He then went under treatment at the Westminster Dispensary for four months, during which period the muscles gradually drew the heel up to the buttock. He afterwards resorted to St. George's Hospital, where he experienced some relief, under the care of Mr. Hawkins, and was discharged at the end of three months. Thence he went to St. Mary's Hospital, and was a patient for more than eight weeks, under the care of Mr. Lane. Subsequently, at the Westminster Hospital, he remained for ten months, under the care of Mr. Guthrie, when he was discharged. After six months' suffering, he was again admitted into the same hospital, and continued under treatment for two months more. At length he sought relief at the Middlesex Hospital; but having remained for three months, without any improvement, he left the hospital. Eventually he came to the Royal Free Hospital, and was placed under the care of Mr. Gant on the 19th of October last.

The joint was found to be uniformly swollen, but not so much as to obscure its outline. A thin unhealthy discharge issued from two or three sinuses which led into the joint, and the patient was much emaciated. His general health having been renovated so far as the local disease and lengthened illness allowed, Mr. Gant excised the joint under chloroform on Oct. 31st. By a transverse and slightly curved incision below the patella, and extending from one condyle of the femur to the other, the joint was fairly laid open. The patella was then removed, although in a sound condition, and a slice of the femur and tibia, both of which were partially bare of cartilage, were sawn off. The femur was most diseased, and a second slice was taken from it, until the rose-coloured bleeding bone was reached. All the diseased bone having been thus got rid of, the surfaces of the femur and tibia were seen to be well adjusted to each other, and the edges of the incision brought together with sutures of fine silver wire. No vessel required ligature. The limb was at once placed upon an extended Macintyre splint, of such a length as to allow of the femur and tibia to be in easy apposition without being much pressed together by the foot-board. Broad slips of adhesive plaster were then

drawn around the limb, a splint applied above and below the knee, and a bandage from the foot upwards, and from the thigh downwards, leaving the knee uncovered, around which were placed strips of wet lint. No side splint was used.

No unfavourable or unusual symptom followed the operation. On the eighth day some hemorrhage occurred, which was arrested by the application of ice.

Dec. 17th. The splint was removed, and union found to have taken place; the limb was replaced; general health much improved.

Jan. 2nd. Union was more firm; health continues to improve, and his trembling agitation has subsided.

27th. Union more firm;. limb replaced on a short posterior splint.

From this time the patient sat up in bed daily, but it was deemed advisable not to allow him to try the leg much. When the weather became warmer, he took gentle exercise, with his leg suspended in a sling. He left the hospital with a sound and useful limb, having been allowed to remain an in-patient until he could be admitted into the Margate Infirmary to thoroughly re-establish his health.

Case 2.—Elizabeth D., aged thirty-three, married, on September 29th, 1853, while walking down a gentle descent, slipped over some rotten pears, and caught the heel of her boot in a grating, whereby the knee was wrenched inwards, causing severe pain. Considerable swelling ensued, which never subsided; but the articulation continued movable for three months, at which time, the patient having scrubbed two or three rooms, more intense pain attacked the joint, which soon became fixed. This crippled state continued without any improvement, although leeches and blisters were used from time to time after the injury. The general health remained unimpaired up to October, 1858, when she experienced severe pain, apparently rheumatic, extending throughout the whole limb. This subsided under treatment, but the practitioner to whom she had last applied candidly told her that the case was incurable otherwise than by amputation; and by his recommendation she came to the Royal Free Hospital on September 12th, 1859.

The joint was considerably and uniformly swollen. Three sinuses, one in the popliteal space, discharged a thin ichorous matter, and the limb was bent towards the buttock, but was movable with a grating noise, such as plainly bespoke destruction of the articular surfaces. The constitutional irritation was proportionate to the six years' suffering the patient had endured. She was not so much emaciated, but passed sleepless nights, trembled, and was always in tears.

On October 19th, chloroform having been administered, Mr. Gant excised the joint by making the same incision as in the preceding case. The head of the tibia was sawn off to the depth of half an inch; a thicker portion of the femur was taken away, and the cancellated structure scooped out for more than an inch, so as entirely to remove all diseased bone. The patella was also removed. The subsequent

steps of the operation and adjustment of the splint were precisely similar to those adopted in the other case.

The general health improved rapidly after this operation. The patient gained flesh and spirits, and progressed without a single unfavourable symptom. At the end of eight weeks the splint was finally removed, and so firm was the anchylosis that no other was applied, the limb being merely bandaged from the toes upwards to prevent any tendency to swelling on first moving about.

The patient could rest her whole weight on the operated limb, and she left the hospital in time to eat her Christmas dinner at home.—*Lancet, Aug.* 4, 1860, *p.* 107.

54.—ON EXCISION OF THE HIP-JOINT.

By JOHN ERICHSEN, Esq., Professor of Surgery and of Clinical Surgery in University College.

Under the term "coxalgia" a great variety of diseases of the bones in the vicinity of the hip-joint are included. And perhaps the advance of conservative surgery has produced no greater improvement in the treatment of disease than by inducing surgeons to make a more careful and exact diagnosis in cases of hip-joint disease than was formerly the case, when no distinction was drawn between the varieties depending on the tissues affected. Now surgeons are, for the purposes of operation, in the habit of diagnosing between cases of disease affecting the soft parts only or the bones only; and further, of distinguishing which bone is primarily or principally attacked.

Under the one term "hip-joint disease" are included several forms, commencing in one or other of three primary pathological conditions.

First, in the *arthritic* form, the soft structures—viz., the ligaments, the synovial membranes, cartilages, &c.—are alone primarily affected, usually with acute inflammation, presenting the symptoms of arthritis elsewhere, the disease usually continuing limited to these structures throughout. Its course frequently tends to abscess. Sometimes, however, the joint recovers without suppuration, but with more or less stiffness, or even complete anchylosis by fibrous adhesions of the opposing surfaces.

The second form—the *femoral*—is that in which the morbid action commences in the upper epiphysis of the femur. The progress of this variety of coxalgia is very insidious; its symptoms are by no means prominent; it generally occurs in young children, and is usually, I believe, of tubercular origin. In it you find the yellow, somewhat soft, friable matter filling up the cancelli of the head and neck of the femur, resembling, and indeed being identical with, tubercular deposit in other organs. This form of coxalgia is accompanied inevitably by the formation of abscess in the soft parts around the joint, commonly on the outside of the thigh, and in the gluteal region; and destruction eventually of the articular surfaces, with

caries of the head of the bone, followed by its displacement, and leading to shortening and distortion of the limb, terminating very generally in the death of the patient from exhaustion and hectic.

The third form is the *acetabular*. Here the disease originates in the pelvic bones. When this, the pelvic aspect of the joint, is the part first affected, abscess invariably forms, sometimes inside the bone, in the pelvic cavity, but occasionally presents externally among the muscles, and points most commonly near the pubes, above Poupart's ligament. In these cases, dislocation, though it is met with now and then, rarely takes place. Sometimes, however, the destruction of the acetabulum is so extensive that the head of the femur penetrates it, and passes into the pelvic cavity. Dislocation is most frequent when the disease has arisen in and has destroyed the head of the femur; so that the head being no longer present, no impediment is offered to the action of the muscles around the joint, and the bone at length sslip from its position in the cavity of the acetabulum.

It is on that form of disease of the hip-joint which originates in, and is chiefly confined to, the head and neck of the femur, and to the root of the great trochanter, that conservative surgery has most influence. It is, therefore, of great importance to be able to diagnose this form from the other two.

Distinctive Characters of the Three Forms of Hip-Disease.—Now, before any question of operating can be entertained, and in only the worst forms and most advanced cases of hip-joint disease can such question be discussed, it is necessary to ascertain to which of the three forms of disease does the case under consideration belong—whether, in other words, it be *acetabular*, and, if so, what is the exact state of the pelvic bones, and which are affected; or *femoral*, and how far the destruction of the head and neck of the femur has proceeded; or whether it be *arthritic*, when operation is seldom allowable.

The determination of this question, which is by no means always easy or even possible, is often much facilitated when we are enabled to watch the case, or have a clear history of its progress previous to any examination. But when we have to deal with an advanced case, of which the account is not clear, and the patient, especially if he be a child, possibly unable to give a succinct history, we may generally, by attention to the following points, arrive at a pretty accurate opinion of the precise extent and seat of the osseous disease. In doing this, the points to be attended to are three in number: 1. The absence or presence of *dislocation;* 2. The existence, position, and extent of sinuses; 3. The condition of exposed bone at the bottom of sinuses, as the pelvic bones usually undergo necrosis, not caries; the contrary being the case with the femur. It is by attention to these points that we may generally arrive at a pretty accurate diagnosis as to the precise state of the hip.

1. *Dislocation.*—In the arthritic form, this rarely occurs. Anchylosis, fibrous generally, now and then osseous, is the usual termination.

In the acetabular form, also, it is an infrequent result, the head of the femur usually remaining unabsorbed; and, though the cartilaginous surfaces disappear, the bone remains in its cavity.

But in the femoral variety dislocation is common, the head of the femur being generally disintegrated; that is, the cancellous structure becomes soft, carious, and breaks up into small fragments, which gradually come away in the discharge; and the ligaments being at the same time destroyed, the remainder of the upper end of the bone slips out of the socket, and lies on the dorsum of the ilium, being dislocated backwards and upwards. The limb is thus shortened to the extent of two or three inches, adducted, and crossed in front of the other, its abduction being impossible. This is the usual course in the femoral variety of hip-joint disease.

2. *Sinuses.*—An attentive examination of the position and direction of these is of great importance in forming an opinion as to the seat of the osseous disease. By passing down them a long probe, we may often ascertain the nature and position of the osseous mischief; but still the amount may be concealed by the fact that sound bone may intervene between the surface and the diseased bone, as when the inner aspect of the great trochanter is affected, or sometimes the diseased or dead bone is covered in by a thick deposit of dense plastic matter, and so the real extent of the mischief in the bone remains obscure.

Situation of Sinuses.—There are three situations in which sinuses are met with, which vary according to their point of origin from the abscess, and the position of the diseased bone: hence it is of importance to note their site and direction. 1. When the sinus opens two or three inches below and a little in front of the great trochanter, about the insertion of the tensor vaginæ femoris muscle, the disease is almost invariably *femoral* in its origin. 2. When in the gluteal region, the sinus may indicate *femoral*, but not unfrequently is dependent on pelvic disease; the acetabulum, or a portion of the dorsum ilii, being the part involved. 3. The sinus may open in the pubic region, either above or below Poupart's ligament. A sinus in this situation is almost certainly diagnostic of disease of the pelvic bones, especially the pubic aspect of the acetabulum. When it appears *above* the ligament, it probably leads to intrapelvic abscess. On the other hand, when the abscess opens *below* Poupart's ligament, there is generally disease of the rami of the pubes or ischium.

The situation of the sinuses, as well as their direction, will thus be found to lead the surgeon to a more accurate diagnosis of the seat and extent of the osseous disease than he could otherwise form. And, indeed, it is only by attention to these circumstances that he is enabled in many cases to form an approximative opinion on these points; for the diseased bone is often so covered in by healthy osseous structure, and by plastic matter, that the probe cannot touch it; or the

sinus may be so tortuous that a straight probe cannot follow its sinuosities.

3. But there is other important information obtainable from an attentive consideration of the situation of the sinuses. It is with reference to the probable nature of the osseous disease. In *femoral* coxalgia, this is almost invariably caries: hence those sinuses that indicate the existence of primary disease of the upper epiphysis of the femur prove this to be of a carious nature; whilst, on the other hand, sinuses occurring in the pubic region and by Poupart's ligament are almost invariably dependent on the presence of necrosed bone; necrosis being the form of disease that affects the acetabulum and pelvic bones.

Cases requiring Operation.—Diseases of the hip-joint may, so far as the question of operation is concerned, be divided into two great classes—those in which no suppuration takes place, however acute the inflammation may have been; and those in which abscess forms.

To the first class belongs the *arthritic* variety of the disease. In this form of coxalgia, the patient commonly recovers with a stiffened or even completely anchylosed, though useful and straight limb. In such cases, I believe that excision is never needed; at least, I have never had occasion to do it, nor have I ever seen a case that seemed to me to justify such a procedure.

The second class of cases—those in which abscess forms—are by far the most numerous. In the great majority of these, however, recovery will take place eventually, under properly conducted medico-surgical treatment. But the recovery in such cases is always so far incomplete that the limb is left much crippled, and often of but little utility. In such cases as these, after years of suffering and of confinement to bed, and after a hard struggle for existence, we find the unfortunate patient left eventually with a limb that is shortened to the extent of from two to four inches, wasted and adducted, with a projecting deformed hip seamed with cicatrices; the remains of the upper epiphysis of the femur being dislocated from the acetabulum, and adherent to the dorsum ilii by firm anchylosis. The limb is unable to support the body, and cannot be extended or the sole of the foot firmly planted on the ground; but it is to a certain extent useful in progression, the patient using it as a kind of paddle to push himself on with, as he limps on the point of the toes. In these cases, it is interesting to observe how Nature compensates for the loss of all abduction and rotary power in the hip by giving an extremely increased degree of mobility to the lumbar vertebræ, so that the patient, in walking, swings the pelvis from these, and thus in a great degree makes up for the loss of the natural movements in the coxo-femoral articulation.

But, though recovery takes place eventually in the great majority of cases of coxalgia that have advanced to suppuration, yet in some, and not a few instances, the patient's constitution becomes unequal

to the drain imposed upon it, and fatal hectic will eventually supervene. This fatal result is the direct consequence of the wasting and exhausting influence of the long continued discharge of pus from masses of carious or necrosed bone too extensive or too deeply seated to be eliminated by the natural actions of the part. It is in such cases as these that conservative surgery steps in, and endeavours to save the patient's life by the removal of the morbid cause that keeps up the discharge which is wasting it away. The object here is simply to save life by the removal of diseased bone. For the same reason—the preservation of life from hectic—that the surgeon amputates in an extreme case of suppurating disorganisation of the knee-joint, he excises in an extreme case of disorganisation of the osseous structures that enter into the formation of the hip-joint, amputation being here too formidable a proceeding to be undertaken; and yet the removal of the diseased bone, the irritation and suppuration from which is rapidly destroying the patient, being an imperative necessity.

The *femoral* coxalgia is that form of the disease that is most amenable to operation. In severe and extreme cases of this variety of hip-joint disease, the upper epiphysis of the femur will be found lying in a state of caries on the dorsum ilii, in a suppurating cavity, with sinuses leading down to it. The pelvic bones are sound; the acetabulum filled by fibroplastic matter of a reparative character, though possibly slightly roughened and necrosed at one lip. The soft structures in the gluteal region are thinned and wasted, the limb inevitably shortened and adducted. In such cases as these, the patient will probably perish, if left to the unaided efforts of nature; or if he recover, after years of suffering, it will be with a limb shortened, deformed, and but little useful. Whilst resection, being limited to the upper end of the femur, or at most to the roughened lip of the acetabulum, as well, is an easy operation, removes the cause of the wasting discharges and hectic, and in no way increases the already existing shortening, as it is limited to that portion of diseased bone which is already lying above and behind the acetabulum, and which, being carious, must disintegrate and crumble away in the discharges before a natural cure without operation could take place.

In the *acetabular* form of coxalgia, the ultimate result is, I believe, inevitably fatal, if the disease is allowed to run its own course unchecked by operation. Large portions of the pelvic bones in and around the acetabulum fall into a state of necrosis; that cavity becomes perforated; and the head of the femur, still lying in it, becomes denuded of its cartilaginous investment, roughened, and carious. Large abscesses, intra- as well as extra-pelvic, form, and the patient dies worn out by hectic, the natural efforts of the part being entirely unavailing to separate and to eliminate such large masses of necrosed bone, deeply lying and covered in as they are by the upper end of the femur.

Until a very recent period, surgeons feared to undertake the remo-

val of large necrosed portions of the pelvic bones, and acetabular disease of the hip-joint was accordingly allowed to run its fatal course unchecked. Mr. Hancock was the first surgeon who undertook the removal of large portions of these bones, and since then the operation has been done several times by others. In no case has, I believe, more extensive disease been removed with a good result than in a girl of the name of Fallowfield, who was sent to me three years ago by my friend Mr. Tweed, and from whom I removed the upper end of the femur, the acetabulum, the ramus of the pubes and of the ischium, a portion of the tuber ischii, and part of the dorsum ilii. This patient, when admitted, was in the last stage of disease, exhausted by the constant discharge, and must inevitably have speedily sunk under the effects of the disease, had no effort been made to remove the cause of the prostration. At the operation, she was so exhausted that it was necessary to have her on the operating-table for some hours before she was sufficiently restored to bear moving into bed. Nevertheless, by free use of stimulants and nourishing food, she made a rapid recovery; and a few months ago, the limb was straight, shortened about two inches; good movement existed at the hip; and she could walk with much ease. Constitutionally, she was in perfect health. In cases such as this, the result, if they are left to themselves, must inevitably be fatal. There is no danger of laying open the pelvic cavity during the removal of these masses of pelvic bone; for, as Mr. Hancock has shown, during the progress of the disease, the fasciæ, muscles, &c., lining the pelvis, become so thickened and infiltrated with plastic matter, that they form an effectual barrier which protects the pelvic cavity.

Method of Operating. The patient lies on the sound side. If the disease is femoral, the gluteal region perforated with sinuses, and the soft parts thinned, the head of the bone lying dislocated on to the dorsum ilii, it suffices to pass a director down one of the chief sinuses leading to the carious bone, and to slit this up. If, however, the bone be more thickly covered, and is not easily reached with the probe, and if the sinuses open on the thigh at some considerable distance from the seat of disease, then a T-shaped incision should be made over the upper end of the femur, so as to expose it. The limb should now be forcibly adducted, rotated inwards and pushed upwards by an assistant, the soft structures separated by a probe-pointed knife from around the upper end of the bone, so that the whole amount of disease may come into view. The carious epiphysis is then cut off with a saw, the soft parts around being protected, if necessary, by means of retractors.

There is one practical question with regard to the amount of the upper end of the femur to be removed, which requires consideration; and that is, Should the bone be sawed through below the great trochanter; or through the neck only, leaving the trochanter? The practice should, I think, differ according to the nature of the disease.

If this is femoral, it is best to take away the great trochanter, as the caries has generally reached its cancellous structure, or it may be infiltrated with tubercle. But if the disease be acetabular, it will suffice to remove the head only, leaving the trochanter, which is not affected in these cases. After removing the head of the femur, the upper end should be examined, and any carious parts gouged out. After the epiphysis of the femur has been removed, the acetabulum must be examined, and any rough or necrosed bone lying at its edge should be gouged away.

In the acetabular form of the disease, the early stages of the operation require to be conducted in the way just described; the incisions, however, being made more freely, but not carried so far forwards as to endanger the anterior crural, or so far back as to wound the sciatic nerve. After the removal of the head of the femur, all loose pieces of necrosed bone are to be taken out, and then, by means of ordinary cutting or gouge forceps, the acetabulum and other diseased osseous structures cut away piecemeal. In doing this, it will be found that the necrosed bone readily separates by means of the finger, or by the hand of a scalpel, from the subjacent soft structures; and the muscles and fasciæ lining the pelvic bones, being thickened and infiltrated with lymph, effectually protect the contained parts from all injury.

The result of excision of the hip-joint has to be considered from two points of view: 1. So far as the mortality immediately referrible to the operation is concerned; and 2. As to the utility of the limb that is left after its performance.

1. As to the fatality directly referrible to the operation itself. On this point it is difficult to speak with absolute accuracy at present, the statistics of all the operations not being before the profession; but, so far as my own experience is concerned, it is as follows. I have operated in seven cases. Of these, not one has proved fatal; three are now well and going about; two I have lost sight of, after their recovery and discharge from the hospital; and two have died, one eleven months and the other two years after the operation, from constitutional disease unconnected with it. When we consider that all these cases were instances of advanced femoral or acetabular coxalgia, which would speedily have proved fatal if not subjected to operation, we may with justice look upon them as successful so far as the preservation of life was concerned. And, in this respect, excision of the head of the femur, or of the hip-joint, stands in a different position to similar operations practised on other joints. The surgeon excises the elbow, shoulder, or ankle, in order to restore a useful limb to the patient. Excision of these joints is a substitute for the loss of the limb by amputation—not, as in the case of the hip, for the loss of the life of the patient by continuance of hectic.

2. As to the utility of the limb left after excision of the head of the femur or the hip-joint. In estimating the utility of the limb left

after excision of the hip-joint, we must compare it with the kind of member that would be left in the event of the patient surviving sufficiently long for a natural cure to result. It would be manifestly absurd to compare a limb which had become seriously crippled, diseased, and shortened, before any operation was undertaken, with one in which no morbid action had ever taken place. So also would it be unjust to compare it with the state of a limb left after a slight attack of coxalgia in which an operation could never have become necessary or been contemplated. But, on comparing the result of operated cases with that of those which recover spontaneously, after caries and destructive disintegration of the upper epiphysis of the femur has existed for years, we shall find that the balance is by no means against those in which excision has been done. As I do not consider the operation necessary, in cases of *arthritic* coxalgia, I do not compare the result of these cases with that of those operated on, but confine myself entirely to those in which there has been destructive bone-disease. In such cases as these, when, after years of prolonged suffering, recovery is at last accomplished by natural means, what is the condition of the limb that is left? Why, more or less completely anchylosed at the hip, wasted, shortened to the extent of two or three inches, partially flexed upon the pelvis, adducted, with the knee possibly stiffened, semiflexed and advanced, the patient just able to put the toes to the ground, without the power of bearing upon or rotating the limb; but when he wishes to turn, twisting the whole pelvis by the aid of the greatly increased mobility of the lumbar spine.

After successful excision, the result is much more satisfactory, as you had an opportunity of witnessing in the boy on whom I had operated three years ago. The principal morbid appearance in the limb is its shortening to the extent usually of from two to three inches —to the extent, indeed, to which the preexisting disorganisation and dislocation of the head of the bone had previously reduced it. It is well nourished, straight, firm, and admits of easy and rapid progression. The anchylosis is fibrous, not osseous. The patient is then enabled to flex the thigh on the pelvis, and to adduct it; but, just as in cases that have undergone a natural cure, the power of external rotation and of abduction are lost, the mobility of the lumbar spine compensating for the loss of these movements.—*British Med. Journal, May* 12, 1860, *p.* 351.

55.—ON THE PATHOLOGY AND TREATMENT OF SCROFULOUS DISEASES OF THE SYNOVIAL MEMBRANE OF JOINTS.

By Richard Barwell, Esq., F.R.C.S., Assistant Surgeon to Charing Cross Hospital.

There is no doubt that even the most chronic, insidious, and painless form of this complaint is, at least in the first stages, an inflammation; and hence, the joint must at once be placed at absolute rest.

Some form of counter-irritant should be employed, and proper internal remedies to ameliorate the constitutional condition. This is not the place to enter into the reasons that would cause the choice of one counter-irritant more than another, but it is desirable to point out the disadvantages attending a certain plan too much in vogue. A patient applying with this disease is generally ordered a splint—perhaps leeches, or a blister; and after a time an issue is made, which is kept poulticed for a month or two, discharging all the while. Now it is evident that nothing can more conduce to the spread of the new tissue, whose encroaching growth constitutes the disease, than warmth and moisture, unless it be an indolent suppurating and granulating sore, such as an issue becomes a week or ten days after its application. If issues are thought desirable, they should be either kept open only for a short time after the separation of the slough, and another be made, or they should be touched again with the caustic potash, and so kept irritable. The former method has its disadvantage in the breadth of surface destroyed, the latter, in the depth to which the sore penetrates, and both in the very great pain produced by the treatment. Some persons have believed, and do still believe, that the amount of benefit is proportionate to the pain. This is a mischievous idea, since it leads to a cruel line of practice. The notion, no doubt, was obtained from the observation, that issues benefit mostly at first, and that in a short time their action becomes null, or absolutely injurious, since the constant discharge weakens the tissues, and tends to produce cell generation all around.

A much better, and a much less painful plan than an issue, is the actual cautery: it is one which I would wish to see much more generally used; but its application requires some practice, skill, and knowledge, since an unskilful use is apt to produce too great a destruction of skin, too deep a furrow, or other evils instead of benefits. If the means be judiciously employed in properly chosen cases, its action is marvellous, and now that chloroform prevents the immediate pain, the objections which caused it to be disused in England have disappeared.

The iron which I employ for this purpose is known as Rust's: it is simply a three-sided prism, mounted by one of its oblong facets to a curved handle. The edges are slightly blunted. An iron of this shape has bulk enough to retain the heat a sufficient length of time, and yet presents a small edge. Two irons ought to be used, as a white, not merely a dull-red, heat is desirable, and thus a change of instruments is necessary. While the patient is being placed under the influence of chloroform, the joint to be cauterized should be protected by strips of plaster, or of moistened lint, a good inch, or an inch and a half broad, leaving narrow intervals between each, where the lines of firing are to run. When anæsthesia is fully established, a white-hot iron is to be slowly, and with gentle pressure, passed three or four times along each space, until the skin be turned into a yellow-brown

horny substance. Let it be particularly observed that the rays ought not to be nearer than an inch; the skin must not be divided by the iron; the heat must be sufficient to char the skin. This mode of using the iron was called by Percy ('Pyrotechnie Chirurgicale,') "cauterization transcurrente." We might name it linear cauterization. Rust, of Vienna, and Bonnet, of Lyons, employed it with great success. My own experience warrants me in speaking of it very highly. In the first place, it gives no pain whatever at the time of application, and when the effect of the chloroform has disappeared, still no pain from the cautery can be felt. Compare this with the torture produced by the potassa fusa, under which the patient will writhe for hours after the application. But not merely is the hot iron painless; it absolutely relieves pain. I applied it in a case of hip-joint disease, in which the patient was suffering so much that he dared not move, his face was anxious, and his nights all but sleepless. After he had recovered from the chloroform, he was laughing, talking, moving about in his bed, and wanting to get up. In a case of knee-joint affection, the same change from pain to painlessness followed the use of the iron, and the patient is now able to walk. The narrow charred lines made by the cautery begin to separate in from five days to a week, and come away between the fourteenth and twenty-first day. They should then be healed as quickly as possible.

The value of pressure in strumous synovitis cannot be too highly estimated, if the proper cases be chosen for its use; such, for instance, as those in which the swelling is soft and sluggish, in which a bursting pain is felt, and in which a sensation of cold is experienced. Dr. Scott, of Bromley, first used this plan with considerable success; he applied mercurials to the joints, and over them strapping plaster. The value of the treatment lies in the pressure. Mercurials are of little or no help; in strumous cases I believe they are even injurious. The *modus operandi* of this agent I believe to be, simply preventing the cells, through want of room, from generating afresh, and, therefore, constraining their organizing force to act in the production of tissue from the cells already formed, or in the absorption of those unfit for this purpose. I know no means at all comparable in their power of resolving strumous synovitis to the use of the actual cautery followed by pressure. If, for instance, on the knee-joint the lines have been drawn parallel to the axis of the limb, they will, on healing, contract very considerably like all scars of burns: and if to the pressure thus produced, be added that of strapping plaster, an almost irresistible power of resolution will have been gained. Indeed, unless the case be too far advanced, or too much supported by a vitiated constitution, it will yield to this treatment in a few weeks. The following case shows the value of this method:—

Jane S., aged six, a pale, strumous girl, has been suffering for twenty months from pain and swelling of the left knee, and was admitted into Charing-Cross Hospital under my care, (through the

kindness of Mr. Hancock,) Dec. 9th, 1859. The knee is a good deal bent, the joint being round and shapeless; the child cries when it is touched, and when any attempt is made to move it.

Dec. 21st. I drew four striæ of linear cauterization, one on either side of the patella, and one an inch beyond each of these, and got the limb into a straight position. On recovering from the chloroform the child expressed no sense of pain, but sat up, and began to play with toys and other children in the ward.

On the third day the skin between the cauterized lines looked red and inflamed. On the fifth, some separation at the edge of the slough had occurred. The horny portion gradually separated, and on the 1st January, 1860, the lines left were simply clear lines of ulcer, which were dressed with zinc ointment, discharged very little, and gradually healed. The knee was much diminished in size; moreover, it could be pressed without producing symptoms of pain. It was, however, still swollen, and on the 23rd January, when the scar was quite sufficiently healed, the knee was tightly strapped, and a roller applied from the foot.

30th. Strapping applied again, and towards the end of the week the child was dressed, and allowed to get about by aid of the sides of the bed and of a chair.

March 6th. The child is able to get about so well that the mother insists on taking her out; promises to bring her to me amongst my out-patients. This she has hitherto not done.

Other cases showing the value of pressure might easily be given. I recorded three in the Lancet for Dec. 20th, 1859. Several others proving the extreme value of this combination have occurred to me.

We now, however, come to a much-neglected question—one which is, perhaps, scarcely comprehended in regard to the treatment of strumous synovitis—viz., is the principle of perfect rest to be exchanged for that of limited motion at any period of the disease? If so, at what period? I say that this question is either not understood or is ignored in England altogether; while, in Lyons, M. Bonnet applied some practical suggestions to the answer.

I must, at the risk even of occupying much space, explain the principles whereon the value of passive motion, shampooing, &c., is based. When the cells, which have formed so abundantly in and around the synovial membrane, cease their generative activity, they remain for an indefinite period more or less passive. We know that the gelatinous condition thus produced may last for years without change, and that, being left undisturbed, the mass will, after a time, become the seat of abscess, cause destruction in surrounding parts, produce caries, &c.; or, in more favourable cases, it will begin to assume a fibroid character, and ultimately unite the two bones by anchylosis. Now, I have no hesitation in saying that, in the large majority of instances, the passive condition may be taken advantage of to induce a much more perfect, frequently an almost complete, cure of the

malady ; and that, by allowing it to continue, a much larger mass of disease is formed, much more tissue (periarticular, cartilaginous, or osseous) is implicated, and, therefore, even if the joint ultimately heal over, the cure is far from being as perfect as it might be made ; and if degeneration set in, the ravages are unnecessarily wide-spread. Moreover, be it remembered, that if even a healthy joint be kept very long immovable, fatty degeneration of the cartilages is apt to come on, and even a sluggish inflammation of the synovial membrane. It must not be, however, for one moment imagined, that I would advocate movement of the joint at all stages of the disease. On the contrary, such treatment can only be beneficial during the above-named passive condition, and when all inflammatory signs have disappeared. Hence it is important to form a correct judgment as to this condition. It is well known that each joint, when inflamed, presents a certain spot in which pain and tenderness are much more marked than elsewhere ; this is in every joint a fixed point—on the hip, behind the great trochanter ; in the knee, the edge of the inner condyle, inside the ligamentum patellæ ; in the elbow, at the back, over the head of the radius, and so on. Now, if the surgeon find that pressure upon these points produces no pain, he may begin to move the limb with perfect confidence and safety; and, as far as my experience goes in this treatment (already pretty considerable), with very great benefit. The movement must not, at the beginning, be violent or long continued ; a slight passive motion, and that rigidly in the normal direction, a certain amount of rubbing and kneading with the hand, and then reapplication of the splint, are all that can be undertaken ; and it may be a long time before it will be safe to abandon the patient's limb to his own management. I have also found it a useful plan, when it can be managed, slightly to change the angle of the splint at each reapplication.

The subject of angle at which the joint would be flexed, would naturally lead us to consider another subject—viz., the value, as an actual remedy for inflammation, of changing any vicious position in which the joint may lie. There is no room for doubting the diminution of the inflammation and of the pain after this has been done ; and amongst my cases are one or two which were clearly benefited by this means. But this subject is too wide, and space too limited, for its discussion here.

The three methods more particularly named in this communication —viz., actual cautery, pressure, and passive motion, are neither original nor new ; but the first and last more particularly are not used so frequently, nor with such discrimination and power as they might be. Much benefit would result from substituting these means for the negative, if not injurious, treatment to which these strumous cases are too often subject.—*Lancet, June* 30, 1860, *p.* 640.

56.—LOOSE CARTILAGE IN THE KNEE-JOINT—REMOVAL.

By W. Fergusson, Esq., F.R.S., Surgeon to King's College Hospital.

There is some difference of opinion amongst surgeons as to the means to be adopted for getting rid of these bodies. Some prefer to remove them at once, whilst others leave them in the areolar tissues. Experience goes to prove that success more usually attends the latter proceeding, and that sometimes most serious consequences have ensued from the former, involving loss of both life and limb. We recollect seeing an hospital patient submitted to Syme's operation for the passage of a loose body from the knee to the subcutaneous areolar tissue, but the surgeon could not resist the desire of removing it altogether. The consequence was that the most violent inflammation set in, in a healthy constitution and robust frame. It went on to suppuration, and the leg had to be removed at the thigh, the patient barely escaping with his life. This is by no means a solitary instance of the bad results of direct removal of the loose cartilage, and hence that proceeding has invested almost every case with uncertainty and fear as to the result of any operative measures for relief. Mr. Syme's and Dr. Goyrand's operation, together with various modifications of it, taking the precaution to leave the loose body beneath the skin, at any rate for a time, we have now seen performed on several occasions, without any bad consequences whatever. Some surgeons endeavour to obtain adhesion at the spot recommended for its detrusion by exciting some inflammation by puncturing the synovial membrane subcutaneously with a lithotome. An example of cure by this method, under Mr. Birkett's care at Guy's Hospital, is recorded in a former "Mirror," ('Lancet,' vol. i., 1856, p. 75.) We have heard Mr. Hilton remark at the same hospital that he has treated one case by subcutaneous section with success, and two without any operation, the bodies being kept stationary, and ultimately, he believes, absorbed.

We had the opportunity of seeing another case treated during the past summer at King's College Hospital, in which the loose cartilage had been present probably for ten years, but most certainly for three. Mr. Fergusson preferred cutting it out direct from the joint by means of a valvular incision. In some observations made on the occasion, he stated that he had cut a loose body directly from the knee-joint several times, and the result had always been successful. He adverted to Mr. Syme's subcutaneous operation, but preferred his own method. Fortunately the present case has turned out tolerably well, but the patient ran very great risks from the extreme severity of the inflammation which followed, synovial fluid and pus flowing from the joint for some days; recovery has ensued with a stiff limb. We have seen recovery take place under precisely similar circumstances, but with firm anchylosis of the joint after a long and tedious illness.

We are indebted to Mr. W. Wickham, house-surgeon of the hospital, for the notes of the following case:—

Thomas H., aged thirty-four, admitted into the above hospital on April 26th, with a loose cartilage in the knee-joint. He stated, on admission, that ten years ago, whilst walking, he was suddenly seized with severe pain in the left knee-joint. It lasted for a few minutes, and then as suddenly left him. This occurred on several occasions for a month or two, when it disappeared. He was not troubled again with it till three years ago. He could now feel the loose cartilage at the inner side of the joint. About six months ago the joint became inflamed, for which leeches and blisters were employed, and he was obliged to keep his bed for a fortnight; and, on getting up, he found great difficulty in walking, and the joint was very stiff. He had another similar attack of inflammation in the joint three months since. On admission, the man's general health was very good; the left knee was rather larger than the right. On manipulation, the loose cartilage could readily be felt on the inner side of the patella. The patient walked with a stiff knee to avoid pain consequent on the loose cartilage getting between the ends of the bones.

May 12th. The patient being placed under the influence of chloroform, Mr. Fergusson removed the loose cartilage, which was about the size and shape of a broad bean. The cartilage having been first brought to the inner side of the patella, and the leg straightened, a valvular incision was made over it, and, after some difficulty, it was removed by means of a scoop. Some synovial fluid escaped during the operation. The wound in the skin was closed, and the limb placed in a M'Intyre splint. The patient was then removed to bed, and the limb swung.

13th. The patient passed a very bad night; complains of great pain in the joint, which is much swollen and inflamed. Ordered sixteen leeches to the joint, and a pill containing three grains of calomel and half a grain of opium every four hours.

15th. He is greatly relieved since the application of the leeches. Ordered poppy fomentations to the joint, and a saline draught with the pills every four hours.

17th. The pain in the joint has returned, and the swelling increased. Twelve more leeches were applied, with much benefit.

18th. He is much better; the wound is now discharging a thin fluid, consisting partly of synovia and partly of pus; the pain is much less, and the swelling has diminished. Poultices were applied to the joint.

29th. The splint was this morning re-applied, with very little inconvenience to the patient, and he did not complain of any pain on applying firm pressure over the joint.

June 21st. The splint was left off to-day, and the whole limb firmly bandaged. The wound in the joint is healed. The swelling in the

knee-joint has greatly subsided; manipulation causes no pain. With the assistance of crutches he is able to walk about the ward.

He was discharged to go into the country on July 12th. At this time there was still some swelling of the joint, with very little motion; he could, however, slightly bend the knee without causing pain.—*Lancet, Oct.* 13, 1860, *p.* 358.

57.—CASE OF DISLOCATION OF THE ULNA FORWARDS AT THE ELBOW, WITHOUT FRACTURE OF THE OLECRANON PROCESS.

By Edwin Canton, Esq., F.R.C.S., Surgeon to the Charing Cross Hospital.

[The occurrence of dislocation of the ulna forwards at the elbow-joint, without a simultaneous fracture of the olecranon process, is so rare, that most authors have denied the possibility of its occurrence; others make no mention of this form of injury. One case of this accident is related by Colson in 1835. Mr. Canton says:]

The following case came under my care in the Charing Cross Hospital:—F. P., aged 40, a somewhat short, slim-built, but muscular man, while driving in a light cart at the rate of seven or eight miles an hour, was thrown out, and instinctively extended his right hand to prevent injury to his head. The weight of the body, however, caused sudden and forcible flexion of the elbow, and at the same time the forearm became twisted in under the chest. On rising, it was found that the elbow was considerably swollen, and the power of moving it entirely lost. When admitted into hospital, the forearm was forcibly flexed, and the hand supinated. The swelling, ecchymosis, and tension around the elbow were so great that it was with difficulty any of the more salient anatomical peculiarities of this part could be recognised,—everything appeared, in every way, so disarranged. The skin covering the inner condyle was stretched to the utmost, and here, over a space about the size of a sixpence, it was to such a degree injured, that a compound state seemed to be momentarily threatened. The antero-posterior and lateral diameters of the joint were increased in extent, and the general swelling was so great as to present a circumference far beyond the normal size of this region. Externally and somewhat anteriorly the cup-like cavity of the radius could be indistinctly distinguished: internally, the condyle was unduly prominent; anteriorly no particular point for diagnosis could be determined on, on account of the state of forcible flexion and great tumefaction there; posteriorly also the swelling was very considerable, but, below it, there existed a depression favouring the view that the ulna was broken immediately below its olecranon process. No median gutter, with lateral elevations to bound it, could be felt.

Attempts were made to rectify the mal-adjustment, but without

success; the efforts, however, could not be longer continued, for, it was obvious that the injury already sustained by the soft parts was so extensive,—the obstacles to be overcome so resistent, and the great likelihood incurred of rendering the case one of the compound kind, forbade further trial, and it was agreed, in consultation, to place the limb at rest on a splint, and to keep the parts cool with an arnica lotion. Within the course, however, of forty-eight hours the tumefaction became still greater; a large slough was forming on the inner side of the joint, and high constitutional irritation having set in, I was obliged to amputate the limb at a sufficient distance above the articulation.

Dissection.—A very careful examination of the elbow was made, under my superintendence, by my pupil, Mr. Edgar Browne, with the following results:—

Bones.—The ulna was dislocated forwards, so that the upper surface of its olecranon process became placed in front of the capitellum humeri, and had thus assumed the position naturally occupied by the head of the radius during flexion of the forearm. The radius was supinated and maintained *in situ naturale*—as regards the ulna—by the coronary and interosseous ligaments being intact.

Ligaments.—Of the anterior ligament, the only part remaining at all perfect was a shreddy portion about the centre; all the rest of it had been torn through. The posterior, and both lateral ligaments were completely divided. The coronary and oblique ligaments were uninjured.

Muscles.—The triceps extensor was detached from all its points of insertion. The supinator radii longus was uninterfered with at its origin; but the two radial extensors of the carpus beneath it were torn away from the surfaces whence they spring. All the muscles which arise from the external condyle—with the exception of the supinator radii brevis, and anconæus—were detached from this process. The only muscle that was torn through at its origin from the internal condyle was the flexor carpi ulnaris.—the olecranon and ulnar portions of it, however, continued intact. No mischief whatever had happened to any other of the pronators and flexors. The biceps and brachialis anticus were put greatly on the stretch.

Blood-vessels.—Though much shifting of their position had necessarily taken place, no vessel of large size had been injured; the sacrifice, however, of smaller ones must have been great—judging from the large amount of blood with which all the soft textures were infiltrated.

Nerves.—The ulnar nerve was torn across where it passes behind the inner condyle. The sheath of the median was distended, and its substance permeated with blood. The other nerves uninjured.—*Dublin Quarterly Journal, Aug.* 1860, *p.* 27.

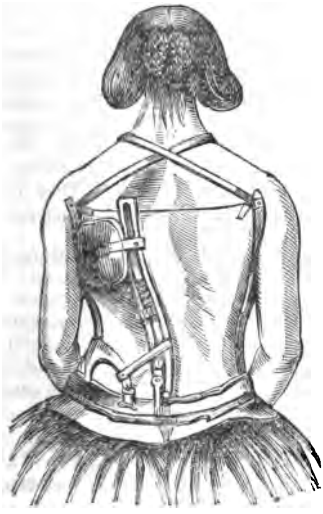
58.—ON THE TREATMENT OF LATERAL CURVATURE OF THE SPINE.

By Holmes Coote, Esq., F.R.C.S., Assistant Surgeon to the Royal Hospital of St. Bartholomew and to the Royal Orthopœdic Hospital.

[After considering the causes and pathology of lateral curvature, showing that after a certain period important changes take place in the bodies and articulating processes of the vertebræ, Mr. Coote proceeds to consider the treatment of the disease. He observes first that for the cure to be effectual it must be done before these changes have taken place.]

The disease commences as one of general weakness of ligaments, just as is seen in cases of knock-knee; but in a brief space of time the opposed osseous surfaces become adapted, and cannot change their form. Of what use then can be cold ablutions, the shower bath, salt and water sponging, or the internal administration of tonics. All the salt and water in the universe will not set straight a distorted back; nor will steel, or other tonic remedies, replace a twisted vertebræ. By mechanical means alone can we hope to check and correct this serious and progressive deformity! This method should be resorted to, not only in the more severe, but in the earlier cases, when hopes may still be held out of even complete and permanent recovery.

Tavernier's belt has been superseded by Tamplin's apparatus, of which a representation is given in the accompanying wood-cut. After many alterations, it is the instrument now commonly used, at the Orthopœdic Hospital. It consists of a steel padded pelvic belt,; of two crutches, capable of elevation to reach the axillæ, and to take off the weight of the head and upper extremities; of a webbing band to pass in front of the abdomen, where, moderately tightly laced, it holds the instrument forwards, and of one or two pads on steel supports, worked with cog-wheels, to press the displaced vertebræ steadily, but slowly back. Patience on the part of the patient, care and skill on the part of the surgeon are necessary, that the apparatus may fit without rubbing, and the weight, which is considerable, may be so equally diffused as to be borne without inconvenience.



It acts under the management of the surgeon, by slow, steady, gradually increasing, yet unremitting pressure, before which the strongest ligaments will yield. The pressure should therefore never be so great as to cause pain, nor to irritate the skin; it must be continued night as well as day, for what progressive improvement could be expected, if an extension of twelve hours were succeeded by a relaxation of a similar space of time; some cases require to be seen twice, others three times a week or oftener; for constant adaptation of the pads, or readjustment of the entire apparatus is necessary during the treatment. These points, however, may be readily learnt by the surgeon, who bears in mind the fundamental rule of patience, steadiness, and gentleness. When the spine has been brought again to the straight line, it can be so held by stays not differing in appearances materially from those usually worn.

The constitutional treatment must be guided by general principles. The patient may take exercise as usual, but should rest on an easy sofa, when tired. She may also with prudence, continue her education, so that the duration of the treatment becomes a matter of less regret to the parents.—*London Med. Review, July* 1860, *p.* 13.

59.—ON THE DRESSING OF STUMPS AFTER AMPUTATIONS.

By F. Symonds, Esq., F.R.C.S., Surgeon to the Radcliffe Infirmary, Oxford.

The chances of quick union and a good stump undoubtedly depend, in a great measure, upon attention to a number of minute details in the dressing. Though the importance of such details has been generally acknowledged, they are too minute and various to have had much space allotted to their consideration in works on surgery. The following observations on certain points connected with the dressing of stumps have no pretension whatever to originality. They are simply a statement of views, which both reason and experience have induced myself. and I believe many others, to adopt.

Sutures.—If we divide sutures into two great classes, viz., Metallic and non-Metallic, the majority of surgeons of the present day—in amputation wounds, and most others where sutures are applicable—will probably be found to give preference to the former. In perfectly healthy tissues, union will generally take place so readily that the kind of suture to be used is not so much a matter for consideration; but when from any cause union is likely to be tardy, then it is a great desideratum to have one which may be kept in the wound for a comparatively long period, at the same time causing the minimum of irritation and lesion of the tissues through which it is passed. This seems to be the very advantage which the metallic suture possesses over one of any organic material.

I must beg to acknowledge the experiments—so conclusive as to the relative merits of these two classes of suture—made some time ago under the direction of Professor Simpson, of Edinburgh (vide 'Medical Times and Gazette,' January 1, 1859.)

Assuming, therefore, the general inferiority of the organic suture, I beg to offer some observations on the relative merits of the two chief kinds of metallic suture—viz., the common wire and the needle (or twisted) suture.

With a view to test their relative value, repeated experiments were made by myself and Mr. Gray our house-surgeon. In different patients, with different descriptions of wounds the twisted suture was used side by side with the wire suture, and the result in almost every case was this—that, at the end of a given time, union was more advanced and more satisfactory at the points where the twisted suture was employed, than at the other points. These experiments appear to me to carry some weight; for, when two kinds of suture, inserted simultaneously, are left side by side in the same wound for an equal length of time, there can be little room for fallacy in one's estimate of their relative efficiency.

The superiority of the needle to the wire is probably due to its greater firmness, enabling it to hold the edges of a wound in more exact apposition. The superiority of wire to thread, on the other hand, must be attributed to its inertness from non-absorption of surrounding fluids.

An objection sometimes urged against the twisted suture is this—that often (in consequence of blood and lymph drying around the points of entrance and exit) its removal requires an amount of force which causes pain to the patient, as well as injury to the newly-formed adhesions. This evil may be prevented by a very simple precaution—well oiling the thread before it is twisted over the needle, and oiling it again (with a camel's-hair brush) once a-day as long as the needle remains in. The needle may then be removed without any pain, or any injurious traction on the tissues recently united.

A very excellent kind of needle for the twisted suture is the common lance-pointed hare-lip needle, made by Weiss. It does not oxidize; it cuts easily, and is easily cut. I have suggested to Messrs. Weiss, as a matter of economy, to have them made at least double their present length, since about a quarter of each one (the flattened extremity) has to be wasted.

Bandaging.—In ordinary cases is it advisable to bandage the stump? I think not. The practice of bandaging the stump immediately after amputation seems to be grounded on the following suppositions: That it prevents retraction and involuntary twitching of the divided muscles, and (according to some) the entrance of air or deleterious fluids into the veins. Now let us examine what truth there is in each of these hypotheses.

1. As a matter of fact does bandaging prevent muscular retraction?

My own observation convinces me that it does not. I doubt whether it can even retard retraction. Consider the case of an amputation of the leg or thigh. The immediate and primary retraction of the divided muscles takes place probably in the course of a very few hours. In the operation the whole thickness of the muscles has been cut across, therefore the whole substance of the remaining muscle contracts. Now any one, who knows any thing of the enormous strength of a single bunch of muscular fibres, will bear in mind how great must be the collective retractile force of the several muscles of a limb acting simultaneously. I am strongly inclined to think it is far too powerful to be counteracted by any amount of bandaging.

Again, even if it were possible to effect this retraction, would it be desirable? Surely not; for until the retraction has taken place, and the muscles have become quiescent, I do not see how Nature's process of repair can commence.

I have spoken of the immediate, primary retraction of the muscles. There is a subsequent, secondary retraction, which begins generally about the second or third week after the amputation, and lasts a considerable time. It shows itself by an alteration, to a variable extent, of the position of the cicatrix. In a thigh, for instance, the undue action of the flexors gradually pulls backwards the line of the cicatrix. Now, according to my experience, bandaging will no more prevent this secondary, than it will the primary retraction. The truth seems to be this,—that if a surgeon has left flap enough to allow for retraction, its occurrence will be no mishap; if he has not left enough, no amount of bandaging will remedy his error.

2. As to the involuntary muscular twitching, causing the stump to start, this I have observed to occur just as much with a bandage as without one.

3. There is one more hypothesis to be considered, viz., that bandaging the stump may prevent the entrance of air or deleterious fluids into the veins from the wound.

This theory ignores the first principles of hydraulics and all that we know of the physiology of the veins, and the circulation. It assumes, first, that notwithstanding an atmospheric pressure of fifteen pounds to the square inch, the cut extremity of a vein may remain open, like a hollow tube, up to the nearest point where the current of blood begins. (We know that in certain localities a cut vein *will* remain open, its connexions being such as to prevent its collapse; but of these we are not now speaking.) Secondly, it assumes that the vein aforesaid has a marvellous power of suction, whereby it is able to imbibe fluid *from* the wound, *through* this tubular portion, *into* the current of blood beyond.

Now, if bandaging cannot answer any one of these ends, and has, further, the disadvantage of complicating the dressing, or concealing the stump from view, of getting constantly fouled by the discharge, of diminishing (when at all tight) the afflux of blood to a part which

needs a very rich supply,—why bandage at all, unless œdema of the stump or some other complication render it necessary to do so?

In my own practice, and that of my colleague Mr. Hester, there have been at this Infirmary a considerable number of cases of amputation, in which primary union took place at all points, except those which gave exit to the ligatures, and good stumps resulted. In these the mode of dressing was as follows:—The wound having been secured with twisted sutures, a single layer of wet lint was placed loosely over the end of the stump, and kept constantly wetted for about a week; no other covering was applied. Then, as each suture was removed, a narrow strip of adhesive plaster was put to support the parts it had held in apposition, and changed as seldem as possible. No bandage or compress was used from first to last.

Plaster.—When it is desirable to support the edges of the wound with adhesive plaster, whatever may be the width of the strips towards their ends, I have found it better not to cut them more than half-an-inch broad at the part which is to be in contact with the wound. The advantages are (1), That the condition of the wound in the intervals between the strips is thereby constantly open to inspection; (2) There is the smallest possible area of contact between wound and plaster,—a great advantage when the former happens to be irritable; (3) Free vent is left for discharge.

The position of the strips of plaster should be changed occasionally, otherwise the intervening parts, from the unequal distribution of pressure, have a tendency to become unduly prominent. Attention to this will secure an evenly-rounded contour to the stump; neglect of it will do the reverse, however well the stump may have been cut. This leads me, lastly, to notice how much it is in the power of the dresser to mould a stump, by making constant, steady pressure (whether by plaster or other means) on any point. If in any case, to use Mr. Paget's terse language, unremitting pressure will cause unrepaired absorption, more especially has it a tendency to do this in a part where new tissue is in process of formation.—*Medical Times and Gazette, Sept.* 8, 1860, *p.* 230.

60.—ON TALIPES EQUINUS.

By Richard Wm. Tamplin, Esq., F.R.C.S., Surgeon to the Royal Orthopœdic Hospital.

[In the first part of his paper Mr. Tamplin makes the startling remark that out of the 20,000 cases of talipes equinus which have passed under his observation, not one was congenital. The most usual causes are irritation with disturbance of the nervous system, as from dentition, worms, and febrile affections; also from wounds, rheumatic affections, scrofulous disease of the bones of the leg. It also sometimes arises spontaneously, without any apparent cause.]

When talipes equinus arises from either of the three first-named causes, it is occasionally associated with paralysis or spasmodic contraction of the corresponding upper extremity, as shown in the statistical report. The majority of the cases, however, are those in which the muscles of the calf alone suffer by contraction. There are some in which the anterior tibial muscle is the only one paralysed. The balance of power being, under these circumstances, destroyed, the extensors appear to contract upon themselves, and thus produce the deformity.

Not unfrequently the first cause has been an attack of hydrocephalus, upon recovery from which the flexor muscles of the foot and toes are found to be perfectly paralysed or interrupted in their functions. I have in some cases had opportunities of witnessing the origin of the deformity. The history is as follows:—The parents of the child notice the frequent tripping and falling of the child from any slight impediment or irregularity on the surface trodden upon. They also notice a divergence of the toes, or, as they term it, increasing breadth of the front of the foot. If you request them to lead the child across the room in these cases, you will notice that the moment the opposite extremity to the one affected is carried forward beyond the perpendicular position of the body, the heel of the affected foot is perceptibly raised from the ground, and the toes unduly diverge from each other, by the increased pressure thrown upon the front part of the foot, and the inability to flex it, owing to the incipient contraction of the gastrocnemius muscle. If the leg be kept extended, it will be found, upon examination, that the foot can be forcibly flexed to its full extent; but, upon withdrawing the hand and setting the foot free, the the gastrocnemius will immediately contract upon itself and draw up the heel. Upon making inquiry, you will generally find that the child is suffering from irritation consequent upon dentition, or from the presence of the small thread-worms.

In these cases, I have had the gums freely lanced; calomel, scammony, and jalap, administered for relief from the worms; stimulant embrocation applied throughout the whole length of the spinal column, and attention paid to the general health; applying to the foot tin splints, bent somewhat beyond a right angle, to counteract the tendency of the gastrocnemius to contract upon itself. I have in many cases followed up this plan of treatment for months; but, although the contraction was at its commencement, the results have been unsatisfactory, except in a very few instances. Division of the tendo Achillis must be had recourse to, and then all tendency to contraction ceases, if proper precautions be used.

Some years since, Dr. Stromeyer, to whom we are indebted for the introduction of this new mode of treatment, visited this Hospital, when a slight case of the kind just alluded to was placed on the table for operation. I consulted him upon it. He advised the use of the hot bath night and morning, together with general treatment, stating that he believed that would cure the child. This advice was followed,

out of respect to so distinguished an authority, but with no satisfactory result. I afterwards divided the tendon, proceeded cautiously with the after treatment, and succeeded in perfecting the cure.

These cases are instructive and important; for it appears that, although there is not decided and permanent contraction, yet the tendency to contract is permanent, and will inevitably produce complete equinus, if no efforts are used to prevent it; and also that, as a rule, general means are insufficient. Within the last few weeks, I found it necessary to operate upon a child with this slight form of contraction, after having for six months tried general means without success. The temporary increased length given to the tendon by the operation in these cases, and the consequently weakened state of the gastrocnemius, although only continuing for a time—*i. e.*, until the tendon has resolved itself into the permanent condition—appears to admit of the anterior tibia exercising its full functions, thus restoring the balance of power between the flexors and extensors of the foot; at least, this is the only satisfactory inference I can draw.

Irritation consequent upon a Puncture or other Wound in the Calf produces at times an active and continuous contraction of the gastrocnemius muscle, which terminates in this deformity, without any apparent loss of power in the flexors of the foot, or any interference with the efforts of the will, so far as can be discovered. This does not shew itself at the time the injury is inflicted, nor until the patient has to all appearance recovered from the immediate effects of the wound, attention being then drawn to it by the inability experienced to flex the foot. This also, if neglected, slowly and certainly terminates in complete talipes equinus, the inherent motion of the ankle-joint remaining unimpaired.

Rheumatism is the next of the causes mentioned as producing this deformity, as well as many other non-congenital deformities, as I shall have to mention. Contraction, when arising from this cause, is found to commence during the active stage of the disease, and seems to be produced by the instinctive efforts of the patient to keep the joint at rest. The gastrocnemius muscles are thus kept in a state of constant contraction, and the flexors of the feet, by the same efforts, are kept constantly relaxed, by which all motion is prevented, and the pain attending this affection is of course thereby mitigated or greatly relieved. The extensors become, therefore, permanently contracted, which is not discovered until the patient is able to stand or take exercise, when the heel or heels are found to be elevated, with total inability to flex the ankle-joint. In these cases, the integrity of the joint suffers by thickening of the synovial membrane, or greater or less alteration in structure, offering an obstinate resistance to the restoration of the natural position of the foot, even after the resistance of the contraction of the muscle has been overcome by the operation; and even after the foot is restored to its normal position, great difficulty is met with in the endeavour to restore motion, requiring the

most unremitting perseverance and attention. The muscles or nerves themselves do not appear to suffer beyond the contracted state of the gastrocnemius, neither during the attack, nor at any period subsequent to it; for, after the removal of the contraction by operation, the voluntary powers over the muscles remain unimpaired. In fact, the deformity may be said to be produced by the exercise of those very powers, as we know the less a joint attacked with rheumatism is subjected to pressure, the less painful it is, and it is only by preserving the extended position of this articulation that this object can be attained, inasmuch as the astragalus is kept very nearly out of the articular cavity.

The next cause is *Scrofulous Disease in the Joint or its neighbourhood, or in the Muscles of the Calf or Tendons of the Leg.* Here, again, the instinctive efforts of the will are the remote cause, and the cicatrix or cicatrices the immediate cause. Many patients have applied with disease in the neighbourhood of the ankle-joint, producing effusion into the joint by irritation or inflammation of the synovial membrane. The joint is kept by the patient perfectly passive in the extended position, hence, again, arises contraction.

In some instances, all the muscles surrounding the joint are kept in a state of active contraction, even with very slight symptoms of disease: so much so, that upon examination you would be led to suppose that they were all permanently contracted, for you can neither flex nor extend the foot, which disguises the real nature of the case. This is also occasioned by the efforts of the patient to prevent motion of the joint in walking. A person recently consulted me respecting his son, a boy aged eight years, in whom this condition of the muscles existed, with very slight swelling of the joint. Upon examination, no trace of motion could be discovered, from the extreme rigidity of all the muscles surrounding the joint. I ordered absolute rest, the leg to be kept in a horizontal posture on a couch—poultices and warm coverings to be applied to the limb. In five weeks, the muscles resumed their passive condition, rendering the cause perfectly evident, viz., disease in the joint.

It is important that this peculiarity in connection with the disease should be borne in mind, as at first sight you might be induced to divide the tendons, which would remove the rigidity of the muscles undoubtedly, but that effect would be but temporary; for as soon as the patient used the foot, the disease would again become a source of pain and irritation, and a second contraction inevitably result. It illustrates, however, one mode by which contraction of the muscles takes place, viz., that these constant instinctive efforts of the will, if not relieved early in the attack by local and general treatment, terminate in permanent contraction. Such a case occurred to me many years since, in a patient aged twenty years, who had suffered from childhood; and in this case I was compelled to divide every

tendon surrounding the joint, with the exception of the flexors of the toes.

When scrofulous disease affects the joint itself, a permanently stiffened condition of the joint must be the result. You can, however, keep the foot at right angles during the time the restorative process is going on, and thus secure the position most favourable to the patient. I have had many cases (young patients) in which the most frightful disease existed, affecting the articulation, and in some the metacarpal bones, in which amputation had been recommended, all of which have yielded to treatment, and have been cured of the disease by rest, warmth, attention to the diet and general health, occupying, it is true, from two to three, and even four years. I mention this, in order that you may be induced to hesitate before you condemn the limbs of young patients to amputation, as is too commonly done; for, from an experience of years, I am convinced that it is an unnecessary step.—*British Med. Journal, July* 7, 1860, *p.* 515.

61.—*A New Trephine.*—Dr. G. A. D. Galt, of Virginia, United States, has devised a new trephine, with the object of avoiding injury of the membranes or substance of the brain. The instrument consists of a truncated cone, with peripheral teeth arranged in a spiral direction, and oblique crown teeth. When it is applied, the peripheral teeth act as a cutting wedge, so long as counteracting pressure acts on the crown teeth; on removal of that pressure by the division of the cranial walls, its tendency is to act on the principle of a screw, but, owing to its conical form and the spiral direction of the peripheral teeth, the action ceases. Dr. Galt says that he has operated on the dead subject with it twenty times, and has never succeeded in wounding the membrane, although he endeavoured to do so. The editor of the 'New York Journal of Medicine,' in noticing the instrument, says that it has been examined by the members of the surgical section of the New York Academy of Medicine, and has, after repeated trials, been found fully to answer the design of the inventor. The instant the crown teeth cut through the internal table, all action ceases, and it is impossible to turn the crown.—*British Med. Journal, June* 16, 1860, *p.* 458.

ORGANS OF CIRCULATION.

62.—NEW MODE OF MAKING PRESSURE IN POPLITEAL ANEURISM.

By Dr. Moffatt.

Dr. Moffatt thus describes an apparatus for making compression in a popliteal aneurism, which is a modification of one already employed by Dr. Fountain. "In a few minutes, with the aid of a carpenter, we

erected a structure consisting of a stick of timber about four inches in thickness by eight in width, one end of which was secured to the top by an upright post of the same dimensions. This post was fastened firmly to the floor, and lashed to the iron cross-bar at the head of the bed. It was about six feet in height, and bevilled at the top to receive the stick first-named, these being firmly nailed together. The large piece of timber, about twelve feet in length, rested at the lower end upon a strong table placed at the foot of the bed, thus forming an inclined plane over the bedstead placed lengthwise beneath it. The patient was then placed upon the bed in the supine position, with his leg slightly flexed, somewhat everted, wrapped in thick layers of cotton, and placed in a long fracture box. A compress made of adhesive plaster wound tightly into a roll about an inch in length and three-eighths of an inch in diameter, was then placed over the femoral at the inferior angle of Scarpa's space. Upon this rested the lower end of a perpendicular piece of wood, about an inch square, the upper end of which was bevilled to meet the inclined plane before described. The pressure was commenced at eight in the morning. Its degree was regulated by drawing the upper end of the perpendicular down the inclined plane, to a greater or less extent as might be required. The hand of the operator was kept upon the stick, and thus secured an equable pressure, even though the patient moved his limb, as he sometimes did a very little. A second compress and upright were placed over the artery as it crosses the horizontal ramus of the pubes, and when the pain from pressure on one was too great to be borne comfortably, the other was used; and thus alternately pressure was kept up until five in the evening, when pulsation could no longer be felt in the tumour. Moderate compression was kept up for eight hours longer, after which the patient was kept for several days quietly on his bed. The great simplicity of this plan of compression, which is always available, must be palpable to every one. That it is far easier for the patient, and unspeakably more so for the operator, cannot be questioned. It would also seem reasonable to infer, that the compression itself, made but at one point, is more uniform and certain than has yet been secured by any other method."—*American Medical Times.—Brit. and Foreign Med.-Chir. Review, Oct.* 1860, *p.* 547.

63.—ON THE TREATMENT OF ANEURISM FROM ANASTOMOSIS BY EXCISION.

By OLIVER PEMBERTON, Esq., Surgeon to the General Hospital, Birmingham.

[The means of treatment selected in the hope of curing "aneurism by anastomosis" may be resolved into three well-marked divisions. These are:]

1. Endeavours to alter the intimate structures of the tumours, so as to render the further development of the arterial network well

nigh impossible, and at the same time to promote its destruction and absorption.

2. The deligation of the principal arterial branches that are distributed within the tumour itself, or of those main trunks that immediately give off the branches that nourish it and pass about its locality.

3. The excision of the disease.

The first division comprehends pressure, the use of refrigerants, of caustic pastes, the injection of astringent fluids, and the introduction of setons and of needles with the view of exciting suppuration. Without, here, more particularly inquiring as to the value of compression, refrigerants, caustics, and the injection of astringent fluids, it may be affirmed that each one of these methods will prove of great utility in certain cases; these will, however, be found to be characterized generally by their comparative insignificance in size, and by their situation on the more solid textures of the frame. They appear to me altogether inadmissible in a case like that which I am about to relate, as their exhibition could only delay the attainment of the end in view. On the other hand, the introduction of setons, of numerous single threads, of needles, and ligatures, with the view of exciting suppuration, or other means of treatment having similar objects to attain—all these will be eminently useful, even should they fall short of effecting a complete cure of the disease. The excitement of suppuration in all parts of one of these vascular growths cannot fail in changing completely the nature of its structure; so that should absorption of the now altered tissue be, from its size, a matter of improbability, at least one great danger—that of hemorrhage—is absent, and other steps to ensure its removal can now be adopted in safety.

Of all the modes of treatment devised for the relief of this formidable disease, perhaps none have resulted in so great disappointment as that in which the ligature of either the main arterial trunks, or their branches supplying the tumour, has been accomplished. The return of blood into the disease in many instances occurred as certainly as the flow through the main channel was cut off, and where the only branch yielding a supply was apparently arrested, a new one, ere long, was found to have arisen near the same place. An exception, however, to this result, as will be seen, must be made in favour of those tumours situated in or near the orbit.

Of forty-five cases, in which the ligature was applied, either on the branches or the principal trunk, the following results were obtained :—

In ten of these cases the branches were ligatured. Of the patients who were submitted to this treatment not one was cured!

In thirty-five the principal trunk was tied. Of this number, fourteen were cured, ten died, nine were unsuccessful, and in two the result was imperfectly given. Further, in reference to situation, of

these thirty-five, six cases were tumours in the orbit; of these, five were cured, and the sixth was reported as being imperfectly so.

It would appear, then, from these observations, that the ligature on the branches has altogether failed, and that where the principal trunks were tied, success was only remarkable when the tumours were within the orbit.

With such evidence of the danger and uncertainty attending this practice in the treatment of these diseases, under what circumstances, let us inquire, are we justified in resorting to it? In the first place, the tumour must be sufficiently formidable as to endanger the life of the patient, should its further increase be unarrested. Secondly, to check hemorrhage. Lastly, with the view of preparing the tumour to undergo, in comparative safety, the most certain of all remedies, (where its situation renders it practicable,) the excision of all the parts entering into its formation.

The excision of the structures constituting aneurism by anastomosis, seems to have impressed, in a very strong degree, the minds of those surgeons who first directed their attention to its relief. "You must not cut into it, but cut it out." This was the opinion held by John Bell, and he vigorously and successfully illustrated its value. J. L. Petit held the same doctrine, expressing it in the important caution that the incisions should be made in the sound tissues, where the bloodvessels were unaffected.

George Freer, who published his "Observations on Aneurism," in in 1807, and who added a lustre to his connexion with the General Hospital in this town that has never been surpassed, by being the first to successfully tie the external iliac artery for the cure of femoral aneurism, also was impressed with the importance of the total removal of these growths by the knife. He records two cases, in which the disease was situated in the cheek, wherein the treatment by excision was attended by complete success.

More recently surgeons have been content to regard the operation by extirpation as the means of treatment that should be adopted when all others have failed. There are no records of late years, that I am aware of, of cases where this mode has been resorted to in the first instance; and yet there are many instances where such a course would have induced far less suffering, and would have placed even life itself in smaller peril. Thus Nélaton relates, that M. Mussey, for the relief of a fungus of the head, tied both common carotids, but was after all obliged to have recourse to excision, and cured his patient.

Dr. Warren's case also (American Journal of Medical Science, 1846) may be perused with advantage, as it shows that however necessary preliminary treatment may be ere excision can be ventured on in safety, it yet must come to it at last. The tumour, in this instance, was situated on the forehead, and was three inches in diameter: after repeated applications of needles and compressing ligatures to the

arteries in the neighbourhood of the disease, together with the use of caustics, the cure had to be completed by excision.

This excision, or complete extirpation of the diseased structure, need not of necessity be carried out by the knife. Sir Benjamin (then Mr.) Brodie effectually accomplished it in the case of a tumour similarly situated, and which had resisted all previous efforts at cure, by means of ligatures so applied around steel needles, that complete stangulation was produced at its base. (Medico-Chirurgical Transactions, vol. xv., p. 177.) The situation, however, of the disease, must be the main guide to the selection of the mode by which it shall be thus entirely removed.

We are justified, then, in arriving at the following conclusions which may regulate our surgical treatment of these cases :—

a. To cut out the parts containing the vascular structure as speedily as possible.

When the lips are the seat of the disease, this proceeding may, in some instances, be adopted, to the exclusion of all other modes.

b. If this excision cannot with safety be accomplished in the first instance, when the disease is otherwise situated, owing to the dangers of hemorrhage, or the prospect of too great disfigurement, the branches of arteries leading into the growth should be tied subcutaneously by needles and ligatures, whilst the intimate structure of the disease may be further altered by the introduction of numerous double threads, in the way I have described.

Should the situation be altogether unfavourable for the use of the knife, Sir Benjamin Brodie's plan of strangulation at the base should be adopted.

By whatever mode, however, the vascular character of the growth is modified, let this change only be desired in the hope that one or other of the means of extirpation I have mentioned may be carried out.—*Lancet, May* 26, 1860, *p.* 516.

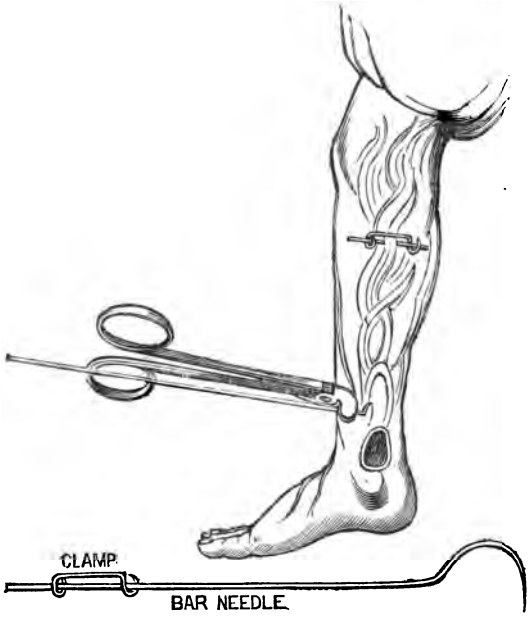
64.—CLINICAL REMARKS ON VARICES, &c., AND THE BAR-NEEDLE AND CLASP.

By JAMES STARTIN, Esq., F.R.C.S., Senior Surgeon to the Hospital for Diseases of the Skin.

[Shortly before Professor Simpson published his papers on acupressure, Mr. Startin was experimenting as to the treatment of varicose veins, He used a tubular needle, through which, having passed it around and behind the vein, he introduced a strong wire ligature, and then, withdrawing the needle, left the wire in situ. Over this, a piece of gutta percha or metal was twisted so as to compress the vein and cause its obliteration. On the introduction of acupressure, Mr. Startin says,]

I immediately saw its adaptation to the end I was endeavouring to accomplish by the tubular needle, and forthwith constructed a needle from a No. 19 knitting-pin,* which I employed with such marked success as induced me to get the instruments which are before you, made by Messrs. Savigny & Co.

They consist of six or eight inches of No. 19, 20, or 21 steel wire, properly tempered, so as to bend and yet break short when required;



one end being ground to a triangular point, and bent into a curved form of different sizes, to the extent of two or more inches of its pointed extremity, so that the needle represents a sickle with a long handle, the curved part being constructed to pass under and around the veins, the other straight portion or stem to form three or more bars. The point of this needle should project the sixth of an inch above the wire of the stem, and, as before said, the temper of the steel requires to be regulated so as to permit it to bend, and yet break short if pressed against any solid resisting body, as the pair of forceps

* Any ordinary workman can convert these pins into the bar-needle, the numbers to be employed being Nos. 18, 19, 20, and 21. They will afterwards require to be properly tempered.

also shown in the diagram. I have found some difficulty in obtaining the exact tempered steel required, so that were it not better to avoid a multiplicity of instruments, it might be preferable to direct the needle to be made soft enough to be divided at the required place by cutting nippers; if such cutting portion could not be added to the present forceps. These forceps, you will observe by the diagram, somewhat resemble the ordinary tooth-forceps, save that their descending blades are made to fit accurately by plane surfaces, excepting where two grooves cross them at acute angles, one of which grooves is to hold the needle for insertion beneath the vein—the other for withdrawing it when its point has passed the skin.

By reflecting on the shape of this needle, it will be seen that when its curvature has passed the vein, that its straight portion will compress the part to which it is applied with considerable force, which I have practically found quite sufficient to intercept the flow of blood through the vein and secure the obstruction of the vessel in all cases where much adipose tissue is not present, or where the vein is of medium size only; but, in the event of either of these occurrences, or indeed, in many instances, for additional security during the first ten or twelve hours, I have latterly used the second part of the contrivance, which I have called the Clasp. This consists of a piece of tinned iron-wire, varying in length and strength to suit the curvature of the needle and consequent length of the bar employed and left beneath the vein. One end of this clasp, as seen in the drawing, consists of a small circular ring or eye, and the other of a hook of corresponding size.

I have now, I fear not too intelligibly, described the Bar-needle and Clasp, a simple, yet, I believe, most efficient apparatus, which, if I mistake not, is destined to occupy as wide a sphere of utility in surgery as glycerine—an introduction of mine sixteen years ago—has held in therapeuties,; for this needle is doubtless applicable to various other purposes than those indicated.

It may be mentioned that this instrument seems to be adapted to the treatment of aneurism by arterial compression; to the operation for the radical cure of hernia for closing the ring; to the treatment of many forms of nævus; and as a readily applied tourniquet, either before operations or after arterial wounds, or hemorrhage. In the latter instance I have once successfully employed it,—viz,, in the case of a schoolmistress residing at Peckham, who is suffering from fungoid tumour of the breast, from which she lost a large amount of blood daily, reducing her to a hopeless condition; three bar-needles, introduced at different parts of the base of the breast, and the clasp applied, have not only arrested the bleeding, but have produced a sloughing of the diseased mass, thus conveying a hint which, when a more favourable case offers, I shall not fail to turn to account. I have also used the bar and clasp with success, instead of Professor Simpson's wire seton, in hydrocele.

Little more than a month has elapsed since I substituted this apparatus for the tubular needle and wire ligature in the treatment of varices, and I have already employed it in nearly thirty cases in public or private practice with a degree of success which has followed no other treatment with which I am acquainted, as the examples about to be cited will testify; but, to prevent disappointment, let me earnestly request those who adopt my suggestions, to take especial care that no complications exist in the cases selected; but that varicose ulcers and eruptions, and varicose veins, pure and simple, are alone to be considered amenable to the treatment recommended, and that any specific or other cachexy will render the procedure worse than useless. I must also beg it will be remembered that as yet I cannot decidedly pronounce on the effects of the bar and clasp upon very deep or chronic indurated ulcers, such as run the round of the London hospitals, and nearly environ the limb; nor has sufficient time yet elapsed, nor have my applications of the instrument been numerous enough to enable me to speak with much certainty, upon its ultimate effect upon varices, and very chronic varicose eruptions, with much cutaneous hypertrophy; such cases, however, will claim my early attention, and a record of the result in future clinical remarks.

The following is the method employed in operating with the apparatus above described:—The patient being placed in a good light, with his limb upon a chair, the bar-needle is to be seized by the forceps, near its curvature, taking care that its stem rests in the horizontal groove of their blades, its spare portion passing between the handles of the forceps so as to be grasped with them by the hand of the operator, as seen in the diagram. The point of the needle is then to be raised with the hand until it becomes nearly perpendicular to the spot at which it is to be inserted, when the needle is to be depressed so to occasion its curve to pass deeply behind the varicose veins and the smaller dilated plexus behind it, when a little pressure with the left hand on the place where the needle projects on the opposite side of the vein, will occasion its point to pass through the skin, when it is to be grasped by the forceps, and a little pushing with one hand and traction with the other, will bring the stem of the needle into the position it is to occupy, *i. e.*, within two inches or thereabouts of its blunt extremity, so as to allow half-an-inch or more to project on each side of the vein, as shown in the diagram. This portion of the needle now requires to be broken off, which is readily done if the steel be properly tempered, by taking the needle's stem between the blades of the forceps, half-an-inch or so from the point at which it first penetrated the skin, when pressure from the left to the right or from above downwards, made by moving the curved end of the needle, will break off the stem at the desired point, and leave the bar behind the vein with about half-an inch projecting out of the skin on either side. The clasp is now to be applied, if required, (see diagram,) by passing

its ringed extremity over one projecting side of the bar, carrying its middle portion, previously bent to fit, and yet gently compress the parts across the vein, when its hooked extremity may be passed behind the other projection of the bar, and the whole apparatus becomes firmly fixed like the pin in a brooch. By this procedure, of course, all circulation in the vein is arrested, and a coagulum forms on both sides of the bar, which may be rendered more or less permanent by retaining the bar a longer or shorter time.

In my first essays with the tubular needle, four, five, or six days were the periods adopted for keeping the ligature inserted; but latterly I have found fifty to sixty hours quite sufficient, whether in the case of an ulcer, eruption, or simple varix, though the last requires the longest insertion of the needle.

As the *modus operandi* of the bar-needle upon an ulcer, is to check very materially all secretion from its surface, so that it may be said to dry up, I have found the most suitable dressing, as it assists in this drying process, to be blotting-paper, which I fold into a thick compress and wet with water, and apply directly to the sore, the pad being large enough to cover all surrounding inflammation; this is secured by the domett bandage before mentioned, the bar-needle being covered with a piece of lint or adhesive plaster, and left between two of the turns of the bandage, which enables the clasp to be taken off the bar, without disturbing the dressings, should it cause pain after ten or twelve hours; and it is perhaps prudent, as a rule, always to attend to this point, as otherwise the wire may cut the skin, and make, while it cures, a wound, though this may be obviated by a pad or plaster under the clasp.

The blotting-paper dressing and the bar may remain forty, fifty, or sixty hours, when the needle must be taken out, and the ulcer dressed again as before described, taking care not to remove any portion of the paper which may adhere to the ulcer, as this forms an artificial scab, which favours the healing process.

If the case be a varix or varicose eruption only, all that will be necessary will be to apply the wet pad of blotting-paper over the site of the needle, and afterwards roll the limb with the domett bandage, which also may be put on wet with warm water, if pain or inflammation be present: and the eruption itself may be painted with the calamine and zinc lotion of the Hospital Pharmacopœia.

Any subsequent uneasiness in the limb or parts affected, may be moderated by passing a compress wet with tepid water around the bandage, and in very irritable patients the clasp may be required to be removed after a few hours. My own experience of this matter leads me to believe that as far as we have yet gone, the treatment of varicose ulcer, pure and simple, is reduced from weeks into days by this operation.—*Med. Times and Gazette, May* 26, 1860, *p.* 517.

ALIMENTARY CANAL.

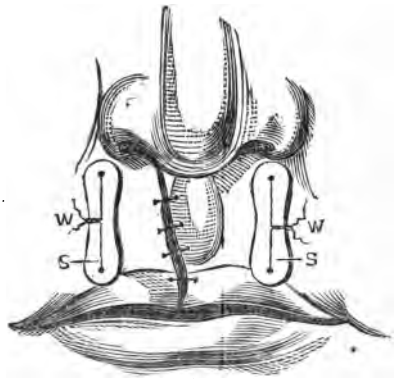
65.—A NEW METHOD OF OPERATING FOR HARE-LIP.

By Edward Woakes, jun., Esq., Luton. (Read before the South Midland Branch British Medical Association.)

[There are some inconveniences attending the ordinary operation for hare-lip; thus, the pins are liable to cause ulceration, and additional support is frequently required, to keep the edges of the wound together.]

The following plan has arisen out of a consideration of the desiderata which a successful operation for hare-lip demands. Briefly stated, these are—1, to support the opposed surfaces of the wound so thoroughly that any amount of contortion on the part of the patient will not interfere with the adhesive process; and 2, at the same time to secure so accurate an adjustment of the skin, that when the cure is completed, there shall remain a minimum amount of cicatrix.

In the operation now proposed, these objects are accomplished in a very simple manner, by substituting silver wires and splints for the steel pins and silk in ordinary use. The wires being introduced, a splint of the size and shape of that represented in the drawing, is



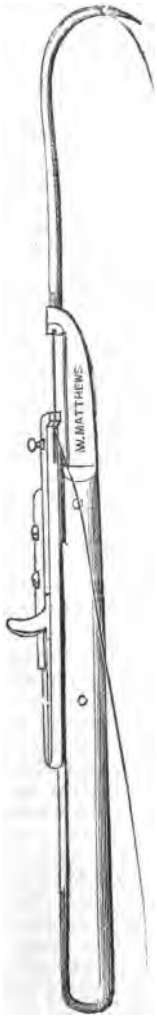
slipped along their projecting ends on each side by means of holes just large enough to admit the wires, and carried home to the cheek, at the spot where the wires were inserted. The ends of the two wires on each side are now twisted outside the splint, which thus serves as a *point d'appui*, from which, at every additional twist, the approximation of the edges of the wound is made more perfect. The chief point to be observed is to insert the wires sufficiently far back to form a firm support to the wound; and this can be done far more safely with

metallic wire than with steel pins, as the former, by adapting itself to the shape of the mouth, causes no painful tension, as do the pins when very deeply inserted. At the same time, so effectual is the support afforded by traction on the splints, that no additional aid for this purpose, such as adhesive straps, spring-trusses, &c., is requisite, even in the most formidable cases. By this immunity from tension, the tissues are enabled to display their well known tolerance of metallic substances, in consequence of which the wires may be retained for an almost indefinite period; whereas, from the oftentimes early commencement of ulceration when the less yielding pins are employed, these may have to be removed before union is complete.

The cut surfaces having been approximated as closely as may be considered advisable by twisting the wires, the line of incision remains still uncovered; and the second condition of the operation may now be accomplished by the insertion of three or more delicate silk sutures very superficially. By this means, the most perfect apposition of the edges is obtained, and in consequence, as regards the final result, a very slight scar—in some cases all but imperceptible—remains. These sutures should be removed within twenty-four hours.

When the operation is complete, the appearance is much that represented in the accompanying drawing, where the actual size and shape of the splints are indicated. The following case may illustrate some undefined points in the operation.

Case.—W. M., aged 18 months, was the subject of congenital hare-lip; the cleft, which extended to just within the nasal orifice, being to the left of the median line. The usual incisions having been made while under chloroform, the wires were introduced about two-thirds of an inch from the line of incision, and made to pass out at a similar distance on the opposite side. This was done by attaching each wire to a piece of silk connected with a straight needle, sufficiently large to admit of the silk, with the wire attached, being drawn through after it. (This mode of introducing metallic wire answers perfectly well when the hollow needle for the purpose is not at hand, care being taken that, where the wire is bent down for the attachment of the silk, it is squeezed together with pincers that it may present as small an obstacle as possible to the passage of the wire at this its bulkiest part.) The ends of the wires to which the silk was united were then removed with scissors, and a splint passed along them, a twist being given to secure it. The other two were then similarly treated; and by alternately twisting the wires of opposite sides with a pair of ordinary pincers, the splints were first brought to lie upon the cheek, and then to approximate the edges of the wound. Three superficial sutures were now introduced, and the integument thus accurately adjusted. Adhesion took place rapidly, and in the course of twenty-four hours one of the superficial sutures was removed. The child, which had not been weaned, sucked well, and appeared perfectly free from pain or uneasiness.



The following day, the other superficial sutures were taken away. On the fifth day, union appeared perfect, and the apparatus was removed; by snipping the wires of one side, and taking off the splint, that of the other with the wires attached was readily withdrawn.

The results of this and subsequent cases, as seen some months after the operation—the traces of the original deformity being less than in the most successful instances of the ordinary mode of operating—may I venture to think, claim for it the attention of those who are interested in plastic surgery.—*British Med. Journal, Aug.* 25, 1860, *p.* 666.

66.—*On a Convenient Needle for the Ready Application of Metallic Sutures in the Operations of Cleft Palate, Vesico-Vaginal Fistulæ, &c.* By P. C. Price, Esq., Surgeon to the Great Northern Hospital.—Those surgeons who are engaged in the renovation of deformities arising from arrest of development—such, for instance, as the deficiency of the soft portions of the roof of the mouth in what is commonly known as cleft or split palate; or in the reparation of breaches of structure, as those resulting in permanent lesions of the female bladder, vagina, and rectum,—have often doubtless experienced the necessity of such mechanical instruments as would enable an easy performance of an operation. Improvements in such necessitous instruments have by degrees been added to the operator's *armamentarium*: but still every addition seems to show the further necessity that exists for yet more useful mechanical aids. Since the introduction of metallic sutures (so ably and forcibly recommended by Dr. Marion Sims) for the more efficient closure of the structures of the bladder, vagina, &c., the want of a suitable means of appliance has been more than ever needed.

The ordinary curved needle set in a handle, and drilled with a single hole near its point is found inconvenient, for great difficulty is experienced in disengaging the metallic thread with

which it is armed. To obviate this inconvenience, among other contrivances, a needle was invented by, I believe, a German, with a moveable point, but still further perfection and simplicity was needed. A short while since Mr. Startin, in the columns of this Journal, suggested the use of a tubular needle, which was a decided improvement, and one that was quickly recognised. Still, however, there seemed room for a more complete instrument to enable a free and ready application of the wire suture in cases rendered unusually difficult of management by reason of the position of the lesion, &c.

While lately witnessing one of Mr. Baker Brown's skilful operations for vesico-vaginal fistula, I was struck with the inconvenience that resulted from the method of applying the metallic suture (which, in this instance, required an extra pair of hands to arm the needle after its insertion through the soft tissues) by means of the ordinary grooved or tubular needle. To dispense with what I considered an impediment in these usually scientific and clever operations, I have devised the needle on the preceding page.

It consists of a tubular needle, such as at first suggested, I believe, by Mr. Startin, set in a wooden handle, but immediately connected therewith by a rod of metal, which is out of the plane common to both the needle and its handle. Working on the upper surface of the handle is a steel slide with a forward and backward movement. At the front end of the slide is a catch-spring, regulated by a small screw-button. The arming of the needle is thus accomplished — The slide, having a moving range of an inch or an inch and a-quarter, is to be drawn as far on to the handle as it will go; one end of the metallic thread to be used is then inserted into the needle at what may be termed its breech, and pushed along the tube till it nearly reaches the point; the portion of wire that projects at the breech is then placed in the catch-spring, and the needle is ready for use.

Having inserted the point, and as much of the needle as is necessary, through the lips of the structure to be closed, the wire is made to project beyond the point of the instrument (as shown by the dotted line in the engraving) by pushing forward with the thumb or forefinger the slide. The projected wire is now caught with a pair of forceps, and disengaged by pressing with the forefinger the button-head which regulates the catch-spring, so that it can be readily and quickly drawn from the needle, or the needle from it.

The instrument is depicted about two-thirds its proper size, and was made for me by the well-known surgical mechanician, Mr. Matthews, of Portugal-street, Lincoln's-inn, and has been much approved of by Mr. Spencer Wells, and other surgeons who are in the habit of performing certain plastic operations on the perineum, &c. In cleft-palate, as well as in operations about the genito-urinary parts, this needle will be found very advantageous, and will at once render the passing of metallic threads through the curtains of the soft palate a less complicated proceeding. It has been my good fortune

to have assisted for many years my friend Mr. Fergusson in nearly all his numerous operations for split palate; and although his master hand makes perfect in utility incomplete surgical instruments, still I have occasionally met with instances in which a more ready appliance for the application of the wire suture (which this distinguished surgeon now usually employs in his palate operations) would have been of much advantage.—*Med. Times and Gazette, June* 9, 1860, *p.* 574.

67.—ON THE REDUCTION OF STRANGULATED HERNIA.

By WALTER JESSOP, Esq., Surgeon to the General Hospital and Dispensary, Cheltenham.

[Mr. Jessop publishes what he thinks a new method of reducing hernia. It is a *good* way, but not a *new* way. The case seemed almost hopeless: an operation was refused, and Mr. Jessop then adopted the following plan:]

Calling a male attendant into the room, I directed my patient still lying on his back, to the edge of the bed, and, with assistance, separated his legs, placing one over each shoulder of the attendant, who, facing the bed, stooped to receive them; and, in this position, by passing his hands round the fore part of the thighs, was enabled to obtain a sufficient purchase to permit of his raising him on to his head and shoulders on the bed, thus throwing the intestines back upon the diaphragm, and to some extent necessarily making traction behind and directly *from* the seat of strangulation. After two or three minutes' manipulation of the abdominal parietes, I found the tumour become less tense, and drawing forwards the integuments round the point of rupture, I made lateral, upward, and downward movements—jerking, as it were, occasionally the parts immediately contiguous to the stricture. This seemed to excite but little suffering; in fact, the patient, so far from uttering complaint, declared himself, after the first two or three minutes, decidedly relieved—that "the dead sickening weight that killed his groin," as he termed it, was better. Continuing these efforts, and varying them as they seemed to occasion distress, I presently felt a slight gurgling under my hand, and almost immediately had the satisfaction of finding the hernia reduced, and my patient comparatively in a state of safety.

The whole proceeding certainly did not occupy ten minutes. Slight peritoneal tenderness existed for some days, but the man eventually did well.

The *rationale* of the proposed plan is simple. A mass, large or small, of displaced intestine or omentum must assuredly be more readily withdrawn from its point of incarceration or strangulation by traction from behind than by the best-directed efforts of the taxis. Any one, for illustration, taking the trouble to put a fold or two of his handkerchief in a ring formed by his finger and thumb, and lightly

strangulating it, will, on attempting to return it by pushing or kneading from before backwards, find infinitely greater difficulty in effecting his purpose than if he were to make traction from behind. In short, the employment of the taxis is at the best a clumsy and most uncertain mode of proceeding, and I shall venture in future to make it merely supplementary to the plan I now advocate.

"One swallow fails to make a summer," and it may be said that the practice of turning patients *à posteriori* upwards is opposed to all orthodox notions of propriety. Admit all this. Others, with greater opportunities, may happily be enabled to add to my single case; and granting that the position of the patient may be accused of positive inelegance, it may, at any rate, contrast favourably with our proceedings in lithotomy and in many other operations on the perineal regions.—*Lancet, Oct.* 29, 1860, *p.* 384.

68.—ON THE USE OF CHLOROFORM IN STRANGULATED HERNIA.

By Thomas Bryant, Esq., F.R.C.S.E., Assistant Surgeon to Guy's Hospital.

[Mr. Bryant has the greatest confidence that in the majority of early cases of inguinal hernia, the hernia can be reduced when the patient has been thoroughly brought under the influence of chloroform. But to effect this, the patient must be rendered completely mastered by the anæsthetic, and a perfect coma, or even stertor, produced, if necessary.]

The following are a few of the cases which have passed under my notice, but they are accurate examples of all others.

A man, aged 41, was admitted under my care into Guy's hospital, on Aug. 3, 1857. He had been the subject of a right scrotal hernia for four years, and had never worn a truss. The hernia had been down twenty-four hours, and symptoms of strangulation had existed during the whole of that period. The taxis had been freely and efficiently applied without benefit, and I was consequently sent for to operate, as no other measures were deemed justifiable. Chloroform was administered, and everything prepared for operation; when, however, the patient had been completely brought under its influence, the taxis was reapplied, and without the slightest force the hernial contents returned into the abdominal cavity: a good recovery ensued.

A man, aged 29, the subject of a congenital hernia of the right scrotum, was admitted under my care Sept. 4, 1859. Symptoms had been present for two days and were very violent. He had been operated upon nine years previously for hernia on the same side. A warm bath, opium, ice locally, and the taxis had been employed without the slightest benefit, and as an immediate operation was indicated, I was sent for. I found the man suffering severely, and the

scrotal tumour was very large, tense, and painful; as it was most desirable to reduce the hernia without operation if possible, I applied the taxis but without success; chloroform was accordingly administered preparative to operation, and when the patient had been thoroughly brought under its influence, the taxis was reapplied; not the slightest force was, however, employed; the hernia contents at once returned into the abdominal cavity, and a speedy convalescence followed.

Many cases like the two quoted might be given, all illustrating the same point, but I believe these are quite sufficient to indicate the benefits of chloroform in the reduction of a strangulated hernia.

I have before me twelve good examples illustrating the benefit of such a practice, all were, however, inguinal. The reduction of a strangulated femoral hernia by the taxis is by no means common; that is, success cannot be anticipated to the same extent, as it may be in an inguinal hernia. In a paper which I published in the Guy's Hospital reports for 1856, I showed that femoral hernia required operation 25 per cent more frequently than inguinal, and subsequent experience has convinced me of the force of such a truth. I also showed that in femoral hernia the taxis is more injurious, and is more frequently followed by gangrenous bowel; such points are worthy of remembrance, as a caution to the surgeon to employ his taxis with discretion and not too forcibly: the administration of chloroform in such cases is beneficial, but the caution as just given should be practically remembered, for force is not indicated, and must do harm, whether employed with or without chloroform.

Acknowledging it, then, as a fact that there are many cases of strangulated hernia which may be returned by the taxis, when the patient has been fully brought under the influence of chloroform; and remembering also that such a practice is demonstrated the most perfectly in those cases in which all other means had entirely failed, and when under ordinary circumstances an immediate operation would have been called for; it may be well to consider how far such a fact should influence our practice, and in what way we can turn such information to account.

A little consideration will soon lead us into the right path; for if it be true that in chloroform we have the most effectual means for producing relaxation of all the muscles of the body as well as of the parts involved in hernia; and if it be also acknowledged that by such means a patient is more speedily and more effectually brought into that relaxed and desirable condition than by any other measures which we have been in the habit of employing, the conclusion to my mind is tolerably obvious, that in all cases we should at once employ the most certain and most efficacious practice, and neither waste time by delay, nor prostrate the powers of our patient by the use of other remedies when we have before us one so incomparably their superior.

The warm bath is doubtless a valuable remedy, and the cases are numerous in which the taxis has succeeded under its influence, which had without entirely failed; but it is nevertheless depressing in its nature, and the depression lasts beyond the period in which such depression is necessary or desirable. The depression associated with a strangulated hernia is very lasting and often very great, and to encourage it by any artificial means cannot be regarded as good practice; and I feel confident that the depressing influence of a hot bath to patients labouring under such a disease as hernia, has often acted most prejudicially to the subsequent success of the case.

Opium, again, is a remedy which cannot be too highly praised, and when given in full doses, and repeated, to bring the patient, who is the subject of a hernia, rapidly under its influence, its useful tendency has been too often manifested to allow us to depreciate its value; but if a surer and more expeditious remedy can be brought forward, and if it can be shown that by the use of chloroform the ends which we have been in the habit of seeking can be attained with greater certainty and without that loss of time which must necessarily take place by internal remedies, and without that lasting depressing influence which is exerted by the hot bath, I think few surgeons will dispute the conclusion, that the administration of chloroform should be primarily selected.

I will not consider the local applications of ice to the hernial tumour, as the cases we are discussing are those of strangulated hernia and not incarcerated, in which the symptoms are marked and not to be mistaken, and in which it is hoped that no surgeon will ever more allow valuable time to pass away by the use of such a precarious and uncertain measure.

When strangulation of the intestines has taken place, it cannot be too often repeated that the more speedily that strangulation is relieved the better; this truth is so recognized by the profession that it may appear too trite for repetition; but let me ask the experience of any hospital surgeon if it is not exceptional for such a truth to be carried out. The cases which appear in hospital practice are, as a rule, such as have been badly treated, and have been most unwarrantably neglected; four, five, six, seven, even ten or twelve days' strangulation is the rule at Guy's Hospital, and some surprise is generally expressed when a case of shorter duration has been admitted. The taxis, opium internally, and ice externally, have been often employed in no unmeasured proportions; and purgatives, even in these days of advanced knowledge, are not exceptional; and it is to prevent such malpractice that I believe a simpler practice would prove most valuable, as timid practitioners would not allow time to pass away by the use of remedies which although of value, are valuable only in select cases, and which as a rule are not to be recommended.

When, then, a strangulated hernia has taken place, what practice should be carried out?

If quite recent, and the symptoms are not severe, a warm bath, a full dose of opium, say two or three grains, and gentle taxis will generally succeed, and in such cases, and such alone, should they be employed.

In others when the hernia has existed for some hours or days, when the vomiting and other symptoms are so marked that there can be no doubt that strangulation of the bowel exists, and not incarceration. I would suggest that the immediate administration of chloroform should be resorted to; let the patient be brought completely under its influence and we may then expect, with considerable confidence, that its reduction by the taxis will rapidly follow, particularly if the hernia be inguinal; the force which is required under such an influence, is simply nominal, anything like power would prove injurious, and would be most certainly unwarrantable; the most moderate application of force is sufficient, if success is to be obtained; and if it should fail no extra power will be more beneficial; let then the taxis be of the most moderate description, let it be firmly and decidedly applied; but do not allow anything like force to be exerted; if success is to follow its application, moderation will suffice; and if failure, force will not succeed.

If failure follows, let the operation be performed; there is no object to be gained by delay, but irreparable mischief must take place, and if the sac is not opened in such cases the operation is not one of danger.

The evils of the taxis, and of delay, as occasioned by carelessness, and the use of remedies, are of such a nature, and have made such an impression on my mind, that I firmly believe that it would be better for all cases of strangulated hernia to be treated at once by the administration of chloroform followed by the gentle taxis, and on this failing by the minor or other operation, than that any chance of delay should be occasioned by the use of remedies, or of injury by the taxis; and that if practitioners would discard all former measures, and adopt those which I have suggested, after experiencing their advantage, I feel sure that the cases of death from strangulated hernia would be but rare, and the cases of mal-treatment would be less numerous.

For if men would only be convinced that by the use of chloroform we have all the advantages of every plan of treatment which has been formerly employed, without any of their evils; that under its influence, if the taxis is to be successful, success will be attained, and free from the danger which attends its use without the anæsthetic,—as the required or permissible force should be but slight; and if they will only be impressed with the truth, that if such means fail, an immediate resort to operation is imperatively demanded, I feel certain that this improved practice would be followed by commensurtae success, and that the mortality of our hospital cases would be materially diminished.

Since the introduction of chloroform the treatment of dislocations

has been completely altered, and the means employed for their reduction are quite simple, when compared with those which were formerly resorted to, viz: pulleys and personal strength.

By the use of chloroform in the treatment of hernia, an improvement equally important is now open to us, and I shall be much surprised if the result does not prove as favourable as I am sanguine enough to believe it will. In both classes of cases the evils of force are dispensed with, and its danger corrected; and in both the influence of science and of manipulative power are well exemplified. But in hernia there is this additional advantage; the delay occasioned by remedies will not be experienced; and I feel convinced that if surgeons will but recognise the value of chloroform as a means of reducing a strangulated hernia, the evils resulting from delay and forcible taxis will be done away with, and a more successful practice must be the result.—*London Med. Review, Aug.* 1860, *p.* 69.

69.—ON ULCERATION OF THE LOWER EXTREMITY OF THE RECTUM; ITS VARIETIES, DIAGNOSIS, AND TREATMENT.

By James Rouse, Esq., Assistant Surgeon to the Westminster Ophthalmic Hospital, &c.

Notwithstanding the numerous works published on diseases of the rectum during the last few years, there appears still to be great difference of opinion as to the best mode of treatment; more particularly with regard to those ulcerations situated on the mucous membrane lining the sphincter ani, and in the fossa immediately above that muscle.

There are three forms of ulceration of the lower extremity of the rectum, which gave rise to very acute suffering; and, although they very considerably in position, have nevertheless been described by most authors under the general head of fissure. It is proposed, in the present paper, to point out that three distinct forms of ulceration occur in this region, which, by ordinary investigation, may be distinguished from each other, and which require different modifications of treatment.

The most common form of ulcer found at the lower extremity of the rectum is that which is known as *fissure of the anus*. This disease does not seem confined to any particular period of life, though it rarely or ever exists until after puberty. It is more particularly common among persons who lead a sedentary life, and for the same reason it is rather more frequent in women than men. The fissure appears to be caused by a tearing of the mucous membrane lining the sphincter ani, by the passage either of hardened fæces, or of a foreign body contained therein. The following cases will, however, show that fissure of the anus may occasionally be the result of external violence:—

Case 1.—A gentleman, aged 24, was riding a restive horse, when it suddenly bolted. He was thrown, with some violence, on the hind part of the saddle before he recovered his seat. He felt some pain about the anus at the time, and, on changing his shirt, he noticed a few drops of blood. For the next few days he experienced a slight burning pain during the evacuation of the bowels, and in about a week the characteristic pain of fissure was established. On an examination being made, a small crack was perceived on the posterior surface of the sphincter; it commenced about two lines within the anus, and extended upwards for about half an inch. Various local means were tried without benefit, and an operation, to be hereafter described, was had recourse to with perfect success.

Case 2.—A captain in the navy fell off a ladder, and came to the ground on his buttocks, with considerable force. He did not observe any particular pain until he went to stool the following morning, when he experienced considerable smarting, and noticed that he had passed a small amount of florid blood. About a week after the accident, he applied for advice. He then, after every evacuation of the bowels, had pain, which lasted for several hours. On examination, an ulcer was found on the posterior surface of the lining membrane of the sphincter; the edges were not indurated, and the surface was florid. An ointment, containing mercury, was applied twice a day; and in the course of a week a cure was effected.

Persons afflicted with this disease, in describing the origin of their suffering, frequently state that while straining violently at stool they felt something give way, and on looking at their evacuations, they noticed a small quantity of blood. It has more than once occurred to me to be told by persons with fissure, that the fæces were so hard that it was necessary to remove them with the fingers from the anus.

This crack or fissure is almost invariably situated on the posterior surface of the sphincter. I have seen upwards of a hundred cases, and in only six did the position vary; in three of these the fissure was situated on the perineal surface of the muscle, and all occurred in women; in two it was situated on the left side; and in one on the right. It commences about three lines from the margin of the anus, and extends upwards in a straight line to the extent, usually, of half an inch, though sometimes as high as the superior margin of the sphincter. If the fissure be seen within a week or ten days of its occurrence, it presents the appearance of a bright red line with a sharply defined edge, and does not appear to extend through the thickness of the mucous membrane. A little later, if no treatment be adopted, one or two florid granulations may frequently be seen protruding above the margin; and it is during this stage that a small amount of blood is voided on going to stool. This appearance is very soon changed; the edges become everted, and more or less hard, and the surface of the ulcer itself looks excavated and pale, like any other indolent sore. The pain caused by this solution of continuity is at

first trifling, and only exists while the motion is passing; but it soon becomes most severe. It usually commences about half an hour after the bowels are relieved (the sensation up to that time being only uneasiness), and continues for five or six hours. As the disease progresses, the pain becomes more continuous and easily excited, and even walking or sneezing will bring it on. At this stage, the ulceration is found to have extended through the submucous cellular tissue into the fibres of the sphincter; there is a constant desire to pass urine, a serious addition to the other suffering, and this continues until relief is obtained by means of an operation.

This second form of ulceration is situated immediately in front of the os coccygis, and was first described by Sir B. Brodie, in a clinical lecture delivered at St. George's Hospital. This ulcer, which is almost invariably coexistent with an enlarged and varicose state of the veins about the rectum, does not, like the one just described, appear to be caused by a tearing of the mucous membrane, but by an injury done to it or to the submucous cellular tissue. It seems as if the forcible passage of the fæces (in these cases always very much hardened), and the pressure caused thereby, produced a small slough of the mucous membrane. Mr. Quain states, in his recent work 'On the Diseases of the Rectum' that he has noticed a case in which, "the disease having been of no long duration, and the suffering comparatively slight, the membrane appeared to be thinned from beneath." The ulcer, once formed, soon increases in size, and usually remains quite superficial for a considerable time; but at length, from the continual irritation, the edges become everted and hard. The surface, however, seldom becomes so indolent as in cases of ordinary fissure; and in this form of the disease the ulceration seldom, if ever, implicates the fibres of the sphincter ani. The pain which, as in fissure, is caused by the evacuation of the bowels is most intense; there is usually very little spasm of the sphincter, but the patient complains of severe lancinating pain, which gradually subsides into a sensation of burning, which continues for three or four hours.

The third form of ulcer is situated in the fossa which exists between the external and internal sphincters; it is by far the most painful and serious affection of the three. It appears to be caused either by the lodgment of a small portion of hardened fæces, or by injury done to the mucous membrane in that situation by the passage of some foreign body, such as a fish-bone. Two cases are known to me where the presence of a polypus of the rectum (the extremity of which was pressed into this fossa every time the bowels were relieved) caused an ulcer in this position.

The ulcer, at first, is seldom more than the eighth of an inch in diameter, and it is generally somewhat deeply excavated. As the disease progresses, the ulceration extends into the substance of the sphincter ani; so that, when the finger is passed into the rectum, the end of it sinks into a small cup-like cavity, the inferior part of which

is formed at the expense of the superior margin of the sphincter Except in cases of long standing, the edges are not indurated, and the surface almost invariably remains florid. In this disease a certain amount of pus and blood is passed at each relief of the bowels. If this ulcer be not cured by means of an operation, it leads to a most troublesome form of stricture of the bowel. The constant irritation, set up by the action of the bowels gives rise to inflammation of the submucous cellular tissue; this causes thickening and hardening, by which means the calibre of the outlet is seriously diminished.

The following case will illustrate this kind of termination :—

Case 3.—Mrs. S., aged 23, complained of very severe pain before, during, and after the relief of her bowels. She had consulted a surgeon, who, on examination, found an ulcer immediately above the external sphincter. An incision was made through the ulcer into the tissue below; but this did not produce the slightest relief. Six months after the operation, she noticed that the discharge was much. increased in amount, and she found more difficulty in passing her motions, which were small and flattened. A year subsequently to the operation, I saw her, and, on examination, discovered an ulcer of considerable size situated on the posterior surface of the rectum, and involving the superior margin of the sphincter, and such extensive thickening of the submucous tissues that the finger could not be passed through. Subsequently, by means of bougies, considerable benefit was obtained.

In these cases, the pain complained of is most severe, and there is more spasm of the sphincter than in simple fissure: in some of these cases the amount of spasm is so great that the muscle increases considerably in size. The pain appears to commence some little time before the bowels are relieved, probably this is caused by the pressure of the loaded bowel upon the ulcer.

Diagnosis.—The diagnosis of these cases is by no means difficult The peculiarity of the pain complained of, the fact of its coming on either during, or soon after, the action of the bowels, and the ease with which these ulcers may be detected by the finger, when it can be introduced into the bowel, render a mistake almost impossible. There exist only two diseases with which these ulcers may be confounded: to wit, a syphilitic ulcer and spasmodic contraction of the sphincter. Neuralgia in the neighbourhood of the sphincter has such well-marked symptoms of its own, that it can scarcely be mistaken. With regard to the syphilitic ulcer, its characteristic appearance, the class of persons affected, the existence of syphilitic ulceration about the vagina, remove all doubts as to the nature of the complaint. The diagnosis between spasmodic contraction of the sphincter and fissure is rather more difficult; in fact, it is only by a most careful examination that the surgeon can determine whether an ulcer exists or not. There are, however, a few points of difference which it would be well to remem-

ber. In spasmodic contraction of the sphincter, the muscle very rapidly increases in size; the anal orifice becomes so contracted that even a gum catheter cannot be introduced without producing extreme suffering. This amount of spasm is most rare in ulceration, and it is the pressure caused by the finger on the ulcer itself that produces the pain. Again, in ulceration, it matters little in which form, sooner or later, there is always discharge of pus and blood; in spasmodic contraction this never occurs. Lastly, the patient having been placed under the influence of chloroform, a careful examination of the bowel can be made (which it is impossible to do without producing insensibility,) and, as in the following case, no ulcer is found to exist.

Case 4.—George ——, aged 45, a man of spare habit, sallow complexion, and depressed vital powers, complained of intense pain, which occurred during the time the bowels were acting, and for several hours after. The pain was not continuous, but came on in paroxysms every few minutes. The motions were very small and flattened; but there was no discharge or appearance of blood. On examination, the sphincter muscle appeared more developed than usual, and the anus was so contracted that it was impossible to introduce the finger. A speculum ani was employed, and the most careful examination failed to discover any ulcer. Under these circumstances, a small bougie, about six inches long, was introduced every other night. At first the pain caused was very great, and he was unable to retain it for more than three or four minutes; but he was soon able to bear it for a longer time. The size of the bougie was gradually increased, and he was ultimately cured.

Case 5.—A gentleman, aged 35, of spare habit and nervous temperament, had suffered with symptoms like those just described, for six months, and the pain had become so severe that he could not take exercise: he had tried various means to obtain relief without success. The most careful examination failed to discover any ulcer, but the sphincter was immensely hypertrophied. Bougies were employed for two weeks without producing the slightest relief, and the patient was so worn out and irritated by the pain he suffered, that he could not be induced to continue the use of them. It was therefore decided to divide the sphincter, and with the exception of the pain produced by the passage of the fæces through the wound, this patient never suffered any inconvenience afterwards.

I should not have insisted so strongly on the existence of this disease, but one of the most recent writers on diseases of the rectum doubts the existence of simple spasmodic constriction of the sphincter.

There is one other precaution necessary in these cases; and that is, to be quite certain that only one ulcer exists. It is not very unfrequent to find two; they may be either one above the other, or situated on opposite sides.

The treatment required for the ulcer in front of the os coccygis, and for fissure, varies according to the stage of the disease. If it be

treated before it has become indolent, local applications, and attention to the state of the bowels, are all that is necessary. Grey oxide of mercury and spermaceti ointment (half a drachm to the ounce) or a scruple of calomel to an ounce of lard, with ablutions night and morning and after each relief of the bowels with yellow soap and water, will usually effect a cure. Great care must be taken in the choice of a laxative, the object being not to purge, but to render the fæces soft, so that as little stretching as possible of the ulcer should take place. Confection of senna or milk of sulphur generally produce the desired effect. A very common medicine in these cases is confection of pepper; this, combined with confection of senna, is very useful in cases of hemorrhoids, but it is apt in all cases of ulceration to produce considerable aggravation of the patient's suffering. When the ulcer has once become indolent, the best and only treatment (likely to prove beneficial) is by the knife. The operation is best performed in the following manner. The patient being placed on the right side, with the knees drawn up to the chin, the forefinger of the left hand is to be introduced into the rectum, and the knife passed up in front of it; the incision is then to be made, commencing a few lines above the superior margin of the ulcer, and to be carried through it down to the external skin, care being taken not to cut into the fibres of the sphincter, except in those cases where the disease has already involved that muscle. After the incision has been made, a small piece of oiled lint may be introduced into the wound. It is better not to allow any action of the bowels to take place for two or three days after the operation; this may be effected by giving small doses of opium or a milk diet.

The treatment required for the ulcer situated above the sphincter is division of the muscle. Local remedies never appear to afford the slightest benefit, but only tend to wear out the patience and spirits of the sufferer. The operation is to be performed in the same way as for fissure; but, instead of merely making an incision into the submucous tissue, the sphincter must be divided by one cut, the wound is then to be dressed in either with oiled lint or silk.

It is of course always prudent to try local means before proceeding to an operation; and the best application is the ointment of grey oxide of mercury, already mentioned. The most satisfactory method of applying the remedy is by means of a suppository tube. The tube should first be lubricated outside, and then filled with the ointment; it is then to be passed into the bowel to the extent of an inch or an inch and a half, and the piston then pushed down; by this means the entire surface of the mucous membrane lining the sphincter is covered by the ointment.

Some surgeons recommend the application of nitrate of silver for these forms of ulceration; but it seldom proves very beneficial, and the pain it causes is quite as severe as that of the division. If it be attempted, a speculum should be introduced into the bowel; by this

means, the ulcer is brought into view, its surface should be dried by a piece of sponge or lint, and the caustic freely applied.—*British Med. Journal, May* 12, 1860, *p.* 356.

ORGANS OF URINE AND GENERATION.

70.—ON INFLAMMATION AND ABSCESS OF THE PROSTATE GLAND.

By R. H. Meade, Esq., F.R.C.S., Senior Surgeon to the Bradford Infirmary.

[Mr. Meade has given us below several very interesting cases, not only to remind us of an operation performed by the late Mr. W. Colles, of Dublin, but to show that such operations as make a communication between the urethra and rectum, are not to be dreaded.]

Case 1.—Towards the end of the year 1854, I was consulted by Mr. H., a man about thirty years of age, on account of a troublesome stricture which had existed for several years, and was then causing considerable disorder of the general health. Upon examination I found a tight constriction in the membranous portion of the urethra, through which a small bougie passed with difficulty. I also noticed a cicatrix in the perineum, and learned that some time previously, he had had a fistulous opening in that situation, through which urine had escaped, but which had gradually healed up. At a short distance from the scar, the patient said he often felt a lump, which disappeared on pressure; and I here found a deep-seated, hard substance, about the size of a hazel-nut, which was evidently a small cavity, communicating with the urethra by a blind fistula. He said that when he made water tolerably freely he felt nothing of this; but when the symptoms of stricture were aggravated, the swelling became full, tense, and painful.

By the careful introduction of bougies during a period of many weeks, I succeeded in curing the stricture, so far that a metallic instrument of No. 10 or 11 size would pass freely into the bladder; and I taught the patient to pass one for himself, directing him to do so regularly once or twice in a fortnight, to prevent the return of the contraction of the canal.

Though he now made water freely, he still complained of very uneasy sensations about the neck of the bladder, in the perineum, and also in the rectum, where he described symptoms similar to those produced by stricture of the gut. The little swelling in the perineum also remained, and he occasionally noticed a purulent discharge along with the urine. To remove this blind fistula, which I hoped might have closed as the stricture was cured, I thought it best to cut into the tumour in the perineum, and form an external opening; on

doing so I found, as I expected, a fistulous passage, along which I could pass a probe for some distance. After this little operation, urine escaped from the opening for a week or two, but the introduction of a probe coated with nitrate of silver, and passed deeply into the sinus, quickly caused its permanent closure.

Upon examination of the rectum with the finger, I found the passage of the bowel impeded by a strong membranous band stretched partly across its anterior surface, at a point opposite to the situation of the prostate gland; which organ appeared much thinned, so that a bougie when passed into the bladder could be felt more distinctly than usual in the rectum. Upon questioning the patient, I found that some time previously to his consulting me, and at the same time that the first fistulous opening formed in the perineum, he had suffered from symptoms of inflammation and suppuration of the prostate; and matter had evidently formed and insinuated itself in various directions, finding one outlet in the perineum, and another into the rectum, after burrowing between the coats of the bowel, and producing inflammation and the formation of the membranous band which I have mentioned. Great portion of the glandular substance of the prostate seemed to have been destroyed, and from the uneasy feeling which still remained about the neck of the bladder, accompanied with occasional discharges of pus, and also from a boggy feeling which existed in the centre of the gland, it seemed very probable that some chronic fistulous passages still remained in the prostate and its neighbourhood.

Before adopting any further operative proceedings for his relief, I wished my patient to consult Mr. Teale, of Leeds; and he suggested that the prostatic portion of the urethra should be laid open into the rectum, an operation which had been recommended and adopted in somewhat similar cases by Mr. W. Colles, of Dublin, some years ago, and which also had been tried by Mr. Teale himself in several instances with success. My patient being anxious to undergo any operation that we could suggest for his relief, I at once recommended him to have this procedure carried out. I introduced a good sized grooved staff of the ordinary curved shape into the bladder, the groove of which could be felt very plainly in the rectum. The patient lying on his side, and the staff being held firmly by my assistant, I then passed a slightly-curved sharp-pointed bistoury (the lower part of which was guarded by some lint) into the bowel upon my forefinger, and thrust the point of it through the centre of the prostate into the groove of the staff, above the membranous band which I have described. I then drew the knife downwards in the groove, divided this band, and laid open the prostatic portion of the urethra into the bowel for about half or three-quarters of an inch. There was very little bleeding from the wound, and no urine appeared to escape from it. The patient was convalescent in a few days, and the operation was completely successful, removing both the obstruction of the bowels and the irritation about the neck of the bladder.

I have seen Mr. H. several times during the last three years, and he continues well, with the exception of being obliged to pass a bougie occasionally. On examination of the rectum with the finger, the gut is quiet free from obstruction.

Case 2.—John A., aged 41, was admitted into the Bradford Infirmary on November 15, 1859, with stricture of the urethra complicated with fistula in perineo, and recto-vesical, or rather recto-urethral fistula. His health was very much broken down, and for several months he had been in a very uncomfortable state; as, whenever he attempted to empty the bladder, the greater portion of the urine escaped by the rectum and through the opening in the perineum. By inquiry into the history of the case, it seemed that about a year before the patient's admission into the Infirmary, he had suffered from symptoms of abscess in or around the prostate, which were relieved by the matter finding its way into the rectum, and were succeeded by the escape of urine from the bowel. About three months subsequently to this, a gathering formed in the perineum, which was opened by a country surgeon, and was followed by a fistulous opening, through which urine also escaped. On the introduction of a bougie, a stricture was found in the membranous portion of the urethra, but not a very tight one, as it admitted a No. 7 or 8-sized instrument without much difficulty. On passing the finger into the rectum the prostate could be felt rather enlarged, but no softening nor bogginess of it could be found. The opening of communication between the bowel and the urethra could be easily felt: it was situated just below the apex of the prostate. The external aperture of the perineal fistula was within half-an-inch of the front margin of the anus, and a probe could be passed from it deeply up by the side of the gut.

After keeping the man in the hospital for a few weeks, for the purpose of improving his general health, and of dilating the stricture by the passage of bougies, I performed the following operation on the 31st of December, 1859:—I introduced a curved and grooved staff into the bladder, and passing a slightly curved sharp-pointed bistoury on the left index finger into the rectum, I thrust the point of it through the lower part of the prostate into the groove of the staff, and cutting downwards laid open the prostatic and part of the membranous portions of the urethra, into the rectum, so as to make a free communication between them. I also divided the sphincter ani, passing a bistoury from the fistulous opening in the perineum through the mucous membrane into the gut, just above the sphincter, and then cutting out. Very little hemorrhage followed the operation. The urine principally escaped by the bowel for the first fortnight, after which it was passed in gradually increasing quantities by the urethra, while the fistulous passage and the wound in the rectum slowly closed. The progress of the case was retarded by the state of the stricture, which seemed to be aggravated by the operation; and it required the frequent use of bougies for several weeks before the contraction and

irritability of the urethra could be overcome. By the expiration of three months, however, from the time of the operation, he was completely cured.

Case 3.—In May, 1855, Mr. B., a gentleman, about 35 years of age, of delicate constitution, who had been under my care several weeks for an attack of gonorrhœa, from which he had almost recovered, was seized, without any apparent cause, with symptoms of inflammation of the prostate gland. He first complained of pain and irritation at the neck of the bladder: but after a couple of days he referred his sufferings chiefly to the rectum; and on examination considerable enlargement and tenderness of the prostate could be felt by the finger in the bowel.

Complete rest, warm hip-baths, leeches both to the perineum and round the anus, were tried, but without producing much benefit; and shivering and other symptoms of suppuration made their appearance. The patient continued to refer most of his uneasy sensations to the rectum, and there was little or no impediment to the passage of the urine, though a frequent inclination to make water. I could feel no hardness nor deep-seated fluctuation in the perineum, but one part of the prostate appearing boggy to the touch when the finger was introduced into the bowel, I determined to make an incision into it at once from the rectum, and thus try to prevent the matter from insinuating itself into the tissues of the perineum, or in other directions. For this purpose I introduced a tubular speculum into the rectum, with a good-sized lateral aperture, which I placed opposite to the middle of the prostate gland. The mucous membrane could be seen projecting into the tube, and distinct fluctuation could now be felt by the point of the finger. I therefore made an incision with the sharpened edge of a gum lancet that had a rounded blade, and after cutting to some depth, about half-an-ounce of thick pus escaped into the speculum. Considerable hemorrhage followed, but it was soon stopped by retaining the speculum (which had a closed extremity) in the bowel, and plugging it with lint. After thus opening the abscess, all the symptoms rapidly subsided, and the patient was soon well. There never seemed to be any discharge of urine from the wound.

Case 4.—In September, 1859, I was called in to see a gentleman, 34 years of age, who had just returned home ill from Scotland. He had recently been married, and had been seized during his wedding trip, while in the highlands of Scotland, with severe pain in the rectum, accompanied with some obstruction of the bowels and difficulty in making water. He obtained slight relief from taking some castor-oil, and made his way as soon as possible to Glasgow, where he consulted a surgeon, who ordered some leeches to the anus, warm baths, opiate suppositaries, &c., and recommended him to travel home as quickly as possible. I found him suffering from a dull pain in the rectum, increased by the sitting posture, from difficulty (but not pain) in emptying the bladder, and also from considerable sympathetic

fever. On introducing my finger into the bowel I found a good deal of enlargement, and great tenderness of the prostate gland. He told me that previous to his marriage he had laboured for some time under stricture of the urethra, from which, however, he had quite recovered; so I have no doubt that there had been subacute inflammatory enlargement of the prostate, such as is so frequently consequent upon long-standing stricture, and this had become aggravated by the excitement of the urinary organs consequent upon marriage, together with neglect of the bowels. I prescribed perfect rest in the recumbent posture, warm hip-baths frequently repeated, salines with hyoscyamus, &c. These measures relieved the pain and general symptoms, but after two days considerable hardness and tenderness appeared on one side of the anus, and the inside of the rectum was very sensitive to the pressure of the finger, a little below the situation of the prostate, showing that the inflammation had spread from the gland to the cellular tissue surrounding the intestine. The prostate itself had somewhat subsided in size, and I could detect no feeling of fluctuation. I I now began to fear that matter had formed, and was insinuating itself in various directions; but on the following day my patient told me that during the night something had burst into the bowel, which had given him great relief, and on feeling an inclination to have an evacuation, he had parted with a small quantity of transparent sticky fluid, something like white of egg, but of a yellow colour, some of which he had saved to show me. With the finger I could now feel a depression and small opening in the mucous membrane of the bowel in the same situation (rather below the prostate) where I found a tender spot the day before. Small quantities of a similar fluid continued to escape for a day or two, and at the same time all the symptoms, both local and general, disappeared. The enlargement of the prostate also greatly subsided.

On looking over these cases, do they suggest any points of special interest to which I would call the reader's attention? In the first place, I may remark that Surgical authors generally seem to have had a great dread of the formation of either a natural or artificial communication between the urethra and rectum. In cases of abscess in the prostate, deep incision is recommended in the perineum, to prevent the matter from finding its way either into the urethra or rectum. I am anxious to show that this fear as regards the latter canal is in a great measure chimerical, and that it is often even advantageous to make a free opening into the prostate from the bowel, in case of suppuration in that organ, should fluctuation be perceptible, as the matter will be so much nearer the surface in that situation than in any other. The diffusion of matter into the surrounding tissues is thus likely to be prevented.

The idea of treating chronic suppuration in the substance of the prostate gland by puncture from the rectum is due to Mr. W. Colles, but he restricted the operation to a particular class of cases. In the

'Dublin Journal' for 1845 he says:—"There is a description of enlarged prostate in which surgery can render essential benefit to the sufferer, and that by a very simple operation. When we find a patient in advanced life complaining of unusual frequency of micturition, with more than ordinary straining, his urine depositing a good deal of muco-purulent sediment, and possibly a muco-purulent discharge from the urethra, we should make a very careful examination of the state of the prostate. If, under these circumstances, we introduce the finger into the rectum, and find the gland enlarged in either lobe, and, upon pressing on one particular spot, we feel the point of the finger sink, as if into a cavity; and particularly if we find this pressure causes the discharge, per urethram, of a quantity of this purulent fluid, to the amount varying from a few drops to a teaspoonful; here we may hope to render an essential service. The operation to which I allude is simply that of striking a lancet into this hollow soft spot, which will generally be found to contain some matter."

Besides those referred to by Mr. Colles, there are many other cases of neglected abscess in or around the prostate in which fistulous passages have formed in various directions, where a free opening between the prostatic urethra and rectum will be especially useful. In both the cases of this kind which I have related, the inflammation and suppuration of the prostate, which laid the foundation for the complicated mischief that existed when I first saw the patients, arose from stricture of the urethra, and occurred in youngish men; and I wish to direct attention to the fact that enlargement, the result of chronic inflammation, very frequently occurs in the prostate in cases of old-standing stricture, and, when not complicated with abscess, is generally easily removed by the internal administration of iodide of potassium, a circumstance pointed out by Sir B. Brodie in his most valuable work upon the Diseases of the Urinary Organs.—*Med. Times and Gazette, Oct.* 20, 1860, *p.* 372.

71.—*Report on the Condition of the Prostate in Old Age, founded on the Dissection of One Hundred Specimens in Individuals over Sixty Years of Age.* By Dr. JOHN COCKBURN MESSER, R.N.—In order to facilitate the consideration of the details of one hundred dissections of the prostate after the age of sixty, the author had arranged them into three classes—viz., 1. Those under four drachms weight. 2. Those between four drachms and six drachms weight. 3. Those over six drachms weight. By so doing, a broad division was at once made between those that were comparatively healthy—namely, the first and second classes; and those that were so altered as to be likely to affect the health of the patient, comprised in the third class.

1. In the first class there were twenty cases, giving—

	Minimum.	Maximum.	Medium.
Age	60 ...	94 ...	76·2
Weight ...	4 drs. ...	6 drs. ...	4 drs. 57 grs.

These cases for the most part, differed from the normal state only in point of size, and offered no obstruction to the flow of urine. The presence of small black concretions was very general in these as well as in all the other cases. In four cases there were slight appearances of the formation of circumscribed tumours. In one case abscess was found associated with stricture of the urethra. In one the posterior lobe showed a tendency to enlargement; but it was difficult to say whether the enlargement was more intimately connected with the prostate or with a fasciculus of the muscular coat of the bladder.

2. In the second class were forty-five cases, which might be considered normal in condition, giving—

	Minimum.	Maximum.	Medium.
Age	60 ...	94 ...	76·2
Weight ...	4 drs. ...	6 drs. ...	4 drs. 55 grs.

None of these cases suffered from urinary obstruction connected with the prostate during life, although the bladder was often found fasciculated. In twelve of these, circumscribed tumours were observed, for the most part only slightly developed; in three, the posterior lobe was slightly enlarged; in one, abscess was present, the consequence of general paralysis.

3. In the third class were thirty-five cases, which gave—

	Minimum.	Maximum.	Medium.
Age	60 ...	87 ...	75·2
Weight	...6drs.15grs....	48 drs. ...	15 drs. 2 grs.

In seventeen of these, the enlargement affected both lateral and posterior lobes; in fourteen, the enlargement existed chiefly in both lateral lobes; in one, the enlargement affected only the left lateral and posterior lobes; in one, enlargement preponderated in the left lateral and posterior lobes; in one, enlargement preponderated in the left lateral lobe; in one, enlargement preponderated in the posterior lobe. Thus it appeared that 35 per cent. of all prostates after the age of sixty are abnormally large, 20 per cent. are abnormally small, and 45 per cent. are within the limits of the normal weight. This enlargement is principally caused by increase of the fibrous element of the body; the glandular also being increased in amount, but not to the same degree. This new fibrous tissue is deposited in concentric layers, and so forms circumscribed tumours. The frequency of this fibrous deposit is shown by the fact that it was present in thirty-four out of thirty-five cases of enlargement, in twenty-seven of which it was found in the form of tumours; in seven there was no appearance of tumours. It also appeared that those glands in which the tumours were marked are liable to the greatest enlargement, as some thus affected were found to weigh thirty drachms, and even forty-eight drachms, while those in which the tumours did not appear never weighed more than seventeen drachms.

A comparison of the relative frequency of enlargement of the different parts of the gland showed that the lateral lobes are much more liable to be affected than the posterior; thirty-four of thirty-five cases were affected in their lateral lobes, while only nineteen of the same number were affected in the posterior lobe. It is rare to find the posterior lobe enlarged while the rest of the gland is normal; only one such case in thirty-five was found. Enlargement of the posterior lobe is the chief cause of obstruction to the flow of urine; but that may also be the consequence of hypertrophy of the lateral lobes, especially when it takes the form of tumours, and they project inwards upon the urethra.

It appears, from the nearly equal average age in all three classes, that the condition of the prostate does not materially affect the longevity of the individual. A slight difference does, however, exist in favour of those in whom the gland is most nearly normal, the average in these being 76·2, and in the enlarged 75·2.

The presence of abscess in the prostate produces enlargement to a greater or less extent, seldom, however, to the same extent as fibrous deposit. The most frequent cause of abscess in the prostate appears to be obstruction to the flow of urine, either from stricture of the urethra, enlargement of the prostate, or the consequence of paralysis of the bladder. The frequency of abscess in the enlarged gland is in the proportion of five in thirty-five; in those between four drachms and six drachms, one in forty-five; in those under four drachms, one in twenty. The causes in these cases were—stricture of urethra in three cases; frequent retention in three cases; paralysis of bladder in one case. Tubercle is the only other abnormal deposit giving rise to enlargement of the prostate noticed in these cases, and that only in one case, which weighed twenty-four drachms. A similar deposit was observed in the lungs, right kidney, and mucous coat of bladder in this subject.

While retention of urine, more or less complete, is the most important symptom and consequence of enlarged prostate, it is not found in every case. The proportion of men in advanced years suffering from the consequences of enlarged prostate is indeed small. Thus amongst 1,600 old men, with an average sick list of two hundred, not more than ten are under treatment for this disease, and half of these only occasionally. A much larger number must be affected with enlargement, as shown by *post mortem* examinations of the gland. In thirty-five cases of enlargement found after death, thirteen suffered no urinary symptoms during life, and two others only after the occurrence of serious lesions to the nervous system shortly before death. Although many of these cases were not greatly enlarged, some of them plainly showed that the prostate may be greatly altered, and yet the patient be free from urinary obstruction, as in one case, where the prostate weighed eight drachms thirty grains, with prominent enlargement of the posterior lobe; in another, which weighed nineteen

drachms thirty grains, with general hypertrophy, and great encroachment on the urethra; in another, which weighed twenty-six drachms thirty grains, with the enlargement principally seated in the lateral lobes.

On considering the favourable circumstances for the formation of phosphatic calculi in cases of enlarged prostate, it is surprising that these concretions are not more frequently found. Of the thirty-five cases of the third class, phosphatic calculi were found in two, the largest weighing seven drachms forty-five grains; in another, two uric acid calculi, of about thirty grains each, were found.—*British Medical Journal, May* 19, 1860, *p.* 383.

72.—ON A NEW FORM OF CATHETER FOR DILATING STRICTURES.

By Buxton Shillitoe, Esq., F.R.C.S.

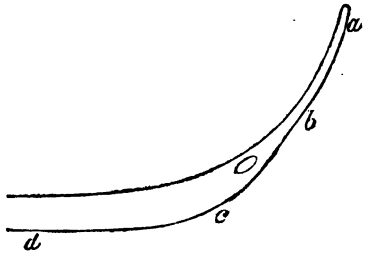
I have often tried, in severe cases of stricture, the old plan of retaining a catheter in the bladder, until I could pass a larger instrument; and I have usually found that if I could once pass a No. 1, or even No. ½, the difficulties of the case were overcome. There were, however, several objections which prevented my adopting this plan as often as I otherwise thought it advisable to do.

Firstly. The time that elapsed before a full-sized catheter could be passed (usually four or five days).

Secondly. The necessity of keeping the patient in the recumbent position during the whole time.

Thirdly. The irritation and soreness induced by keeping the catheter in so long.

Fourthly. The frequent change of the instrument as the dilatation advanced, often produced so much pain as to cause the discontinuance of the treatment before the cure was complete.



Fifthly. The longer the time occupied in the dilatation, the greater the discharge, and the greater the difficulty in getting rid of it afterwards.

These objections, strengthened by the observation that I could frequently skip one or two numbers, even after the first six or eight hours, induced me to try a catheter having the form of a short cone, commencing about one inch and a-quarter from the end. This figure is a diagram of it. The extremity is made rather smaller than need be, and there should also be a hole near *a*, so that urine may flow through as soon as it has passed into the bladder.

The first part, from *a* to *b*, is of the size of a No. 1 catheter; it then gradually increases in the conical portion from *b* to *c*, up to the size of a No. 6 catheter, and this size is continued along the remainder of the tube. My second size commences at No. 6, and after the first inch and a-quarter increases, in the next inch up to 10 or 12.

My first trial of an instrument constructed on this principle was made on a gentleman who had suffered from stricture for the last twenty years. During this time he had submitted to internal division, and the stricture had once (eighteen months since) being dilated to No. 8, by tying in the catheter. This occupied four days. For the four previous months, a No. 1 or No. 2 gum-elastic bougie had been passed frequently, but whenever a No. 3 was tried, it was invariably followed by retention. This case, therefore, did not appear to be a very favourable one. I commenced on Thursday evening, at eleven o'clock, having previously ascertained that the seat of the stricture was near the bulbous portion of the urethra. I passed the catheter carefully through the stricture, and soon found the conical part firmly grasped by it; the urine, however, passed through the catheter. I then tied in the catheter, so as to exert a little pressure upon it, sufficient to prevent its slipping out again, and gave him a dose of opium. I visited my patient the next morning at nine, when I found that the catheter had passed up to the handle, so that the portion of the diameter of No. 6 must have gone through the stricture. I then withdrew it, and passed the one commencing with No. 6; this I tied in firmly, and left it in until eight in the evening, when I was obliged to withdraw it, the patient being attacked by diarrhœa, to which he was very liable, and which was uncontrolled by astringents and opium. Previously to withdrawing it, I exerted a slight amount of pressure upon the instrument, and succeeded in passing it fairly through the stricture, so that in twenty-one hours I had succeeded in dilating it from No. 2 to No. 10. My patient did not suffer from retention or spasm, and but very slightly from soreness. I did not make any attempt to pass the instrument again for nine days. I then passed the larger sized catheter. Not the slightest pressure or force was used, and I succeeded in passing it through as far as No. 8 or 9; if I had used a very slight degree of force it would have gone through, but I did not like to run the risk of producing retention as the patient was compelled to transact business for some hours afterwards. I expect that by passing the instrument about once a-week, and letting the patient sleep with it tied in, once every fortnight or three

weeks, a cure will eventually be made; and I believe, that whenever strictures are capable of dilatation, this plan will be found to involve the least loss of time, the least pain, and the least inconvenience: and whether it be used by tying in the catheter, or by simply passing it, the conical form will be found to dilate more quickly and to be withdrawn more easily, on account of its shape. The part where the cone commences will vary with the seat of stricture, but any instrument maker can easily furnish the requisite catheter. Where this plan does not cure, it will at least be free from risk or danger, and will quickly convert an unmanageable stricture into a perfectly manageable one; or if time is of great importance, the tying in of the instrument for one night per week will dilate the stricture, without encroaching upon the next day.—*Medical Times and Gazette, July 7, 1860, p. 5.*

73.—*New Operation for Phymosis.* By M. Ridreau.—The well-known operation for phymosis, practised by M. Ricord, leaves scarcely anything to be desired under ordinary circumstances, at least in the opinion of the majority. Some, however, object to the permanent exposure of the glans which so extensive a removal of the foreskin entails. To meet the views of surgeons holding this opinion, we quote from the 'Journal of Practical Medicine and Surgery,' the description of an operation, designed and successfully practised by M. Ridreau, a French military surgeon.

"Stretch the prepuce by drawing the mucous membrane forward, and the skin back, so as to lay bare the orifice of the foreskin; introduce a slender cylindrico-conic wooden rod into the aperture of the prepuce; perform a circular incision at about half a line from the mucous margin, dividing the skin only, which immediately shrinks backward on the glans; maintain the mucous lining upon the wooden rod, and remove circularly a sufficient quantity of it to give free play to the glans in the aperture resulting from the operation. Join the edges of the wound of the skin and of the mucous membrane by a few small needles and twisted suture. If a vessel bleeds, apply one of the sutures on that spot."

The wound heals in a few days with water dressing, and then the condition of the organ is perfectly normal, the glans being covered or exposed at will. Examination of the anatomy of the parts explains the success of this operation. The constriction is seated in the mucous membrane, and this is removed. Moreover, the skin of the penis unites with the mucous membrane, not by a diminution of its substance, but by accommodating itself by numerous wrinkles (in the usual manner of skin surrounding the sphincters) to the destined aperture; accordingly, the moment it is divided circularly, it may, without difficulty, be drawn back upon the penis. This operation possesses advantages peculiar to itself; a very limited portion of the textures is

removed, a covering for the glans is retained; no deformity results; the cicatrix is linear, so imperceptible as to be mistaken for the natural junction of the skin and mucous membrane, and is entirely concealed when the prepuce is drawn forward upon the glans; the portion of mucous membrane removed being replaced by integument. —*Dub. Hospital Gazette, Aug.* 1, 1860, *p.* 233.

74.—TWO CASES OF STRICTURE OF THE URETHRA SUCCESSFULLY TREATED BY THE URETHROTOME DILATOR.

(Under the care of JOHN WOOD, Esq., King's College Hospital.)

Two cases of stricture of the urethra were operated upon by Mr. Wood, on the 16th of May, with his new instrument, the "urethrotome dilator." The first of these cases was that of a man of about forty, who had been suffering from stricture for many years, and had never had a larger instrument than a No. 4 passed. The stricture was situated three or four inches from the meatus, and admitted a No. 2 catheter pretty readily. The second case was that of a gold-digger lately returned from California, where he first became afflicted with stricture, the result of gonorrhœa. In this case the urethra was very irritable, the stricture being situated in the most common locality—namely, at the junction of the bulbous with the membranous portion of the urethra. A No. 4 catheter was passed by Mr. Wood, after much careful manipulation.

The instrument which Mr. Wood has but very lately brought under the notice of the profession combines the method of dilatation with that of the internal division of strictures, and consists—

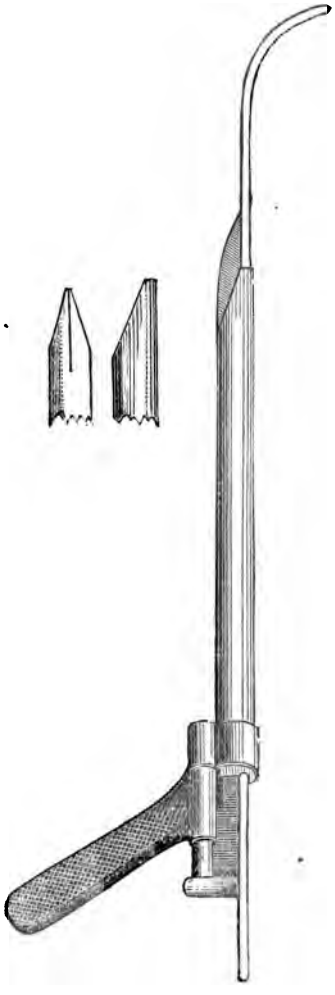
1. Of a long steel staff, of about the calibre of a No. 2 catheter, grooved along its convexity, except for about two inches at its point, where it is curved to that extent.

2. Of a German silver canula, of the size of a No. 12, conical at its point, on the under surface of which there is a slit about half an inch in length. This canula is fitted with a convenient handle, fixed at an oblique angle.

3. Of a flat steel stilette, the point of which consists of a lancet-shaped cutting edge, about three-quarters of an inch long, working in the groove of the director by means of a thumb-plate and spiral spring.

The method of using this instrument seems to be simple and easy of application. The steel director is first passed through the stricture into the bladder, which fact may be ascertained by the urine trickling through the groove. The position of its point is always indicated by the groove, which is situated on the convexity of the instrument, and the short curve at its point allows it to be turned in any direction. The dilating canula is then slid over the director down to the stric-

ture, which its conical point enters for a certain distance. Then the stilette is slid within the canula down the groove of the director, and on pressing the finger-plate at its extremity the cutting edge is protruded for about a quarter of an inch through the slit at the point of the canula, and still sliding in the groove in the director. It returns within the canula by the spiral spring in the handle extremity of the instrument. The dilating canula is now pushed on, and if it does not pass through the stricture, this may be scarified at its inferior, lateral, or even superior aspects, and the dilator pushed through it. The dilating and cutting portions of the instrument are now withdrawn, still leaving the original director. Over this again an elastic catheter is passed into the bladder, and the director withdrawn through it. In the two cases in which Mr. Wood operated, the instrument answered perfectly, the operation being performed without the aid of chloroform. In the first case there was no bleeding, and in the second only a few drops of blood escaped. Little pain was experienced by the patients, who walked home shortly afterwards, suffering little uneasiness.



Mr. Wood, in his remarks after the operation, made reference to the relative value of his instrument with those for the internal division of strictures from behind, in which a comparatively large-sized instrument must be passed through the stricture befor-

scarification can be had recourse to. He said that the chief recommendations of the urethrotome dilator were—1. The safety with which it might be used. 2. That the cutting portion of the instrument would only cut the indurated and contracted tissues of the stricture, and only just sufficient for the passage of the dilating canula. 3. That all previous dilatation of the stricture became unnecessary. 4. That it affords a ready means of combining dilatation with limited section of the stricture.

May 19th. The second patient presented himself to-day, having suffered scarcely any inconvenience from the operation, since which the urine has been passed more freely, and in a larger stream. A No. 8, then a No. 9, and finally a No. 10 catheters were passed with ease.

The particulars of these two cases were furnished by Mr. Charles S. Mathews, assistant house-surgeon to the hospital.—*Lancet, July* 21, 1860, *p.* 58.

75.—ON ALLARTON'S OPERATION FOR STONE IN THE BLADDER.

By Barnard Holt, Esq., F.R.C.S., Surgeon to the Westminster Hospital.

[This operation has often been declared to be improper, when the stone, or the prostate, are large. It would appear, however, that these two objections are not good ones, as the following cases show.]

J. R., a child aged eight years, was admitted into the Westminster Hospital under my care, April, 1860, suffering from stone in the bladder. The symptoms had existed for eight months prior to his admission, and consisted in difficulty and frequency of micturition, which required considerable straining to effect; occasional passage of bloody urine; and, but rarely, pain at the extremity of the penis. The boy having been placed under the influence of chloroform, a sound was passed to the neck of the bladder, where its further progress was arrested by some foreign body, which a slight amount of pressure sufficed to displace. The sound being thus fairly introduced, a stone was immediately detected, which, from the extent of surface traversed, appeared to be large.

I decided on performing Mr. Allarton's operation. On May 8th, while under chloroform, a staff as large as the urethra would admit was introduced, and the stone immediately detected. The child was now secured in the ordinary manner, and the forefinger of the left hand being passed into the rectum, the exact situation of the prostate was ascertained. A sharpe-pointed and somewhat triangular knife was now thrust into the mesian line of the perinæum, with the back towards and about three lines in front of the anus, the finger in the rectum guiding the knife, and preventing its being wounded. An

incision was now made directly upwards, and the groove of the staff cut into by opening the urethra immediately in front of the prostate; and the knife was now run backwards and forwards to a slight extent so that it might be fairly divided, the tegumentary opening being enlarged as much as appeared necessary in its withdrawal. The forefinger was now removed from the rectum, and passed into the wound, with the nail in the groove of the staff, which thus formed an accurate guide for the director, which, being made with a handle at right angles, was passed along the groove into the bladder. The assistant was now directed to pull the staff gently but firmly upwards towards the symphysis pubis, whilst I pulled the director gently downwards, and the forefinger of the left hand, being passed between these two instruments, was gently wormed between them with a twisting motion, dilating the parts as it proceeded until the neck of the bladder was reached, when both staff and director were withdrawn, and the dilatation was continued still further by rotating and moving the finger from side to side. The forceps were now introduced upon the finger, which was withdrawn as they passed into the bladder, so that to the present time no urine had escaped. The stone having been detected, the blades of the forceps were separated, when the gush of urine carried it between them, and with gentle traction it was removed. As was anticipated, it proved of considerable size, measuring two inches long and one inch wide, and weighing 240 grains; it was of the triple phosphate variety. The hemorrhage was of the most trifling description, and beyond the smarting attendant upon a wound he hardly suffered anything. *There was no involuntary escape of urine* after the operation, but it was passed at will, for the first week through the wound, but afterwards per urethram. Eight days from the operation he was walking about the ward, and in a fortnight was discharged cured.

The second case occurred in a spare, feeble man, aged sixty-five, who had suffered from stone for four years prior to his admission. The urethra admitted a full-sized sound, and the prostate was perfectly healthy. The bladder was, however, considerably contracted, not holding more than four ounces of urine; and the stone, which was readily detected, was large and dense. The same proceedings were adopted as have been already described; but the stone, proving even larger than was anticipated, eluded the blades of the forceps. A large pair were consequently had recourse to, and the stone removed. It was found to measure two inches and a quarter long by one inch and a half in breadth, and weighed 570 grains. It was of the oxalate of lime variety, coated with phosphates. The patient was comfortable during the evening, and passed his urine at will. He was, however, seized in the morning with a rigor, and died suddenly.

The post-mortem was made twenty-eight hours after death, and the bladder having been removed, the incision was found to be exactly in front of the prostate. The bladder was now cut into, and the mucous

membrane corresponding to the neck was found to be congested and slightly torn. not, however, extending into the prostate gland, and much less than is met with in the lateral operation. The kidneys were in an advanced stage of cystic disease; the heart fatty; but the other organs were healthy.

The third case occurred in my private practice, in the person of a gentlemen aged seventy-five, of spare habit, who had suffered from diseased prostate more or less for the last eight years, during which time he had occasional hemorrhage, sometimes slight, and sometimes sufficiently severe to call for the exhibition of gallic acid, the bleeding being at all times provoked by any extra walking or jolting exercise. Although there was no pain at the extremity of the penis after micturition, yet the symptoms to some extent, simulated those of stone, and the bladder was from time to time carefully examined both by myself and others, without, however, detecting any foreign body. Early in 1859, while at his seat in the country, he was attacked with inflammation of the bladder, characterized by rigors, great frequency of micturition, spasms of a most violent character, and occasional pain at the extremity of the penis, the urine depositing the usual amount of muco-purulent, tenacious deposit. Suitable remedies were had recourse to, and in the course of three months he recovered; the spasms entirely subsided, the urine regained its healthy character, but the difficulty of expulsion had so far increased as to prevent his passing more than an ounce at a time. He was therefore directed to pass his own catheter three or four times in the day and night, which he accomplished without either difficulty or suffering. In the winter of 1859, being again in the country, he had another but more severe attack, which apparently arose from standing on damp ground, and it was evident, from a sudden discharge of pus and extreme tenderness of the prostate, that an abscess of that gland had formed and burst. His symptoms were now very distressing, the bladder much more irritable, and the pain at the extremity of the penis more decided; and being now unable to pass any urine naturally, he was compelled to rely entirely upon the catheter, which was passed every three hours. The bowels were daily relieved by enemata. The horizontal position was maintained, and all the known remedies had recourse to, belladonna, copaiba, and chlorodyne giving the greatest relief.

In the spring of the present year he was removed to town, the frequent and almost irresistible urgencies continuing, and the pain at the extremity of the penis being more acute. Opium, in the form of suppositories, was the only drug that now afforded the slightest relief.

On Sunday, June 15th, after violently straining at stool, the bladder became more than usually irritable; and being requested to pass his gum catheter, I immediately detected a calculus, rough, and so far as could be ascertained by such an instrument, not very large. A consultation with Mr. Arnott and Mr. Cæsar Hawkins was held: and as lithotrity offered no chances of relief, Allarton's operation was decided upon.

On June 27th he was placed under the influence of chloroform, and the operation, as already detailed, was performed. Not the slightest difficulty was experienced in reaching the neck of the bladder. but the size of the prostate prevented the interior being explored, or the stone being felt with the finger. The forceps were consequently substituted, and being buried to a considerable depth, a stone was caught and removed, proving large, smooth, and polished. Another was sought for and removed, which bore an accurate resemblance to the first. It was, therefore, evident there must be a third, which, after slight difficulty, was also included between the blades of the forceps, but being larger than either of the former ones, it required some little patience and gentle traction to remove it. The parts, however, yielded, and the third, somewhat roughened, stone was now abstracted. The first measured $1\frac{3}{8}$ in. by $1\frac{1}{2}$ in.; the second, $1\frac{3}{8}$ in. by $1\frac{3}{4}$ in.; and the third, $1\frac{5}{8}$ in. by $1\frac{1}{2}$ in.: the weight of the three being 960 grains. The incision was insignificantly small; the hemorrhage very trifling; and my patient being now placed in bed, shortly recovered from the effects of the chloroform, and simply complained of the slight smarting of the wound. The neck of the bladder retained its contractility, and there was not any involuntary escape. The suppository was introduced as usual; and, excepting when the urine was withdrawn, he slept tranquilly. For the first three days he progressed in the most favourable manner; the intervals were increased, the catheter being introduced every four, instead of every three, hours; the spasms were less frequent and less intense, and he was nearly relieved from the pain he had previously experienced, when, on the fourth day, he was seized with rigors, extreme and urgent pain in the region of the stomach, vomiting, and his pulse, which was exceedingly weak, rose to 130; skin hot; countenance anxious; and the urine presenting an appearance of bloody jelly. Opium and stimulants were administered, the bladder was injected with tepid water, and he rallied. He, however, continued exceedingly ill for three or four days. when the attack gradually subsided, the urine became more natural, but the spasms of the bladder recurred; and fearing the possibility of a calculus having descended from the kidney, he was again examined, under the influence of chloroform, without however discovering any foreign body; a roughness was detected with the gum-elastic catheter, but not with the sound, and in all probability depended upon a calcareous coating of some portion of the mucous membrane; indeed, this was subsequently ascertained to be the case, for upon introducing a lithotrity spoon, a portion of gritty matter was removed, and eventually the corresponding mucous surface, coated with phosphates, came away in the eye of the catheter. Small particles of the same deposit have from time to time escaped. The frequency and spasms have now entirely subsided; he is enabled to pass per urethram from two to three ounces of urine; the bladder will retain from ten to twelve ounces; the wound has so

far healed as to be hardly perceptible, and when necessary he can introduce his own catheter. The bowels which have hitherto required enemata of water, have acted naturally since taking the confection of senna; and an examination per anum proves the prostate to be diminished in size. He is daily gaining flesh and strength, and when the weather will permit drives out.

The present cases have been described simply as examples to prove that large calculi and an enlarged prostate form no bar to the operation, and, as far as they go, establish the facility with which a large stone can be removed without injury to the neck of the bladder. It is true that the mortality is about the average; but as in the fatal case there was probably sufficient cause in the state of the kidneys and heart superadded to the rigor, from which persons in advanced years frequently die, I cannot consider it as in any manner militating against the operation.

The case of the boy was interesting in showing how circumstances may alter and modify symptoms generally relied upon as characteristic of stone. The pain at the extremity of the penis was the exception and not the rule, and evidently depended upon the stone generally occupying the same position and requiring to be displaced before the sound could be fairly introduced. The diagnosis was correct in reference to its size, which it must be admitted very far exceeded the average of such cases at such an age.

In the second case, the facility with which the stone was detected, and the ringing sound, afforded sufficient evidence of its size and density to preclude any advantage from lithotrity.

The third case must be considered as the most important of the three. The size of the prostate—which did not permit the finger to reach the bladder after the necessary incisions had been made, the long duration of the disease, the inability to pass any urine without the introduction of a catheter, the irritable state of the bladder, the emaciated condition of my patient, and the presence of three such very large calculi, were sufficient to test the operation to the utmost. I may mention that this gentleman had been subject to severe bilious attacks which very much resembled the one that supervened on the fourth day, and that the issue of the case proves the symptoms to have depended upon a deranged liver.

I have described the operation as it was performed; and although the precaution of introducing the finger between the director and the sound may not be absolutely necessary, yet I am convinced it simplifies the operation to such a degree that no surgeon of common intelligence need fear undertaking it. The extraction of the stone requires care and gentleness. A minute lost is a minute gained, if a large calculus can be removed through the neck of the bladder without injury; and I feel convinced this can always be effected if the necessary care is exercised. In almost all the descriptions of the ordinary forms of operating, the direction is, to have a large external wound.

Such is not necessary; and if a stone can be removed, without bruising, through a moderate wound, it is certainly better than making a large one.

From all that I have yet seen of Mr. Allarton's operation, I consider it a most valuable one; the wound is less; the hemorrhage is less; the constitutional disturbance is less; there is no chance of infiltration of urine; and, as a matter of comfort to the patient, the ability to retain the urine is of the utmost value and importance.—*Lancet, Sept.* 8, 1860, *p.* 234.

76.—*On Allarton's Lithotomy Operation.* By NATHANIEL WARD, Esq., F.R.C.S., Assistant Surgeon to the London Hospital.—[The lateral operation generally requires much preliminary study, and constant caution and care, in order to execute it with precision and without risk, the chief dangers being the slipping of the instrument from the groove of the staff, and the wounding the bladder or rectum.]

The comparatively greater safety, simplicity, and facility of the operation recommended by Mr. Allarton appear to me to constitute its peculiar merits. On the six occasions on which it has been performed at the London Hospital, either by my colleagues, Messrs. Critchett and Gowlland, or by myself, the steps recommended by Mr. Allarton have been followed with but a slight variation. The staff having been introduced into the *uninjected* bladder, the prostate was made out by the left index finger passed into the rectum, and its apex was steadied by it against the staff. A scalpel, with the blade half an inch longer than the length of the finger in the rectum, and having a straight cutting edge and curved back, was introduced with the cutting edge upwards into the perineal raphé, about half an inch in front of the anus, and passed steadily downwards and backwards until its point impinged in the groove of the staff, cutting through the lower wall of the back part of the membranous portion of the urethra, just in front of the point of the index finger.

The knife was then carried forward about the eighth of an inch along the groove of the staff, and was then withdrawn by sweeping it upwards and forwards (as regards the operator), so as to make a median cutaneous incision about an inch in length. A common bone probe, set in a rough handle, was next passed along the groove of the staff into the bladder. The staff was then removed, and the left index finger, well greased, being passed along the *upper* surface of the probe to the prostatic urethra, arrived through this, after several graduated rotations, into the vesical cavity. The forceps were next introduced along the upper surface of the finger, and the calculi removed.

Thus it will be seen that the only points in which this lithotomy differed from Mr. Allarton's directions were—the use of the form of scalpel alluded to, and the incision of the membranous urethra by

drawing the knife forwards and upwards, instead of passing it towards the prostate in the contrary direction. The cases were all successful, and the patients rapidly recovered.

Independently of the many recommendatory points in this method of urethral lithotomy insisted on by Mr. Allarton, the *necessity of injecting the bladder* before cutting *does not exist*, inasmuch as the cavity of the viscus is not cut *into;* and the bladder is not in danger of being wounded, as the incision nearest to it is carried away from it towards the operator. But there is an important reason why *the bladder should not be injected*, and it is this:—The prostate gland is taken to be, so to speak, the land-mark of the operation. This organ in children is very small, and it requires a little education of the finger in order readily to detect it. If the bladder is tense in consequence of retained urine or injected fluid, the walls encroach on the immediate area of the gland, and the prostate is then not easy to be felt. On the contrary, when that viscus is not distended, the prostate stands out in comparatively prominent relief, and can easily be felt by the finger, and steadied against the staff.

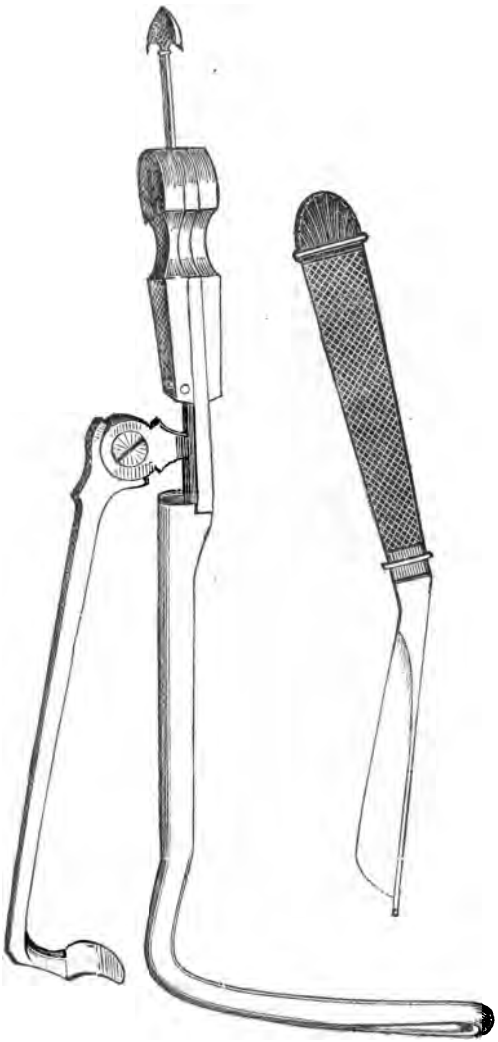
I have little doubt that Allarton's operation will, in course of time, be generally adopted for the removal of small or average-volumed calculi from the bladder, and I cannot but think that since its introduction it would have received ere this a greater amount of sound surgical sanction had some lithotomists of the day, of the highest authority, deviated, in a spirit of inquiry, from a beaten track; and experimentally tested the applicability and efficiency of this modified Marian operation, the revival of which must be regarded as a useful addition to the resources of the healing art, and as a high compliment on the part of modern to ancient surgery.—*Lancet, June* 9, 1860, *p.* 568.

77.—ON LITHOTOMY.

By Professor N. R. Smith, of the University of Maryland.

[Instruments at all complicated only increase the difficulty of this important operation, which is becoming more and more simplified, especially since Allarton's operation has become popular in this country. Nevertheless, we think it right to let our readers see what other people do, and especially our cousins over the water, who are making rapid strides both in surgery and in *journalism* to overtake the mother country. Professor Smith, of Baltimore, has used this instrument with great success for thirty years, in more than 100 cases. He describes it as follows:]

The staff is a silver canula, with a slightly-spiral fenestrum cut out of its dorsum, and continued to near its point; along this the knife glides. You see the cutting director attached to the handle of the instrument by a strong hinge. This has an angle about an inch from



its lower extremity, and at its end is a cutting point, like that of a broad scalpel. On the inferior border of this angular portion is a groove, which receives this, back of the knife. When, as the hinge allows, you extend the director, it stands out at right angles to the shaft. When you depress it, you see the scalpel point enters the fenestrum of the staff at the dorsum, and cannot deviate from it. In the hollow of the staff is a short cylinder, concave towards the handle. To the centre of the concave end is attached a fine, flexible steel wire, which passes up through the handle of the instrument along a small canal. The cylinder glides freely along the canal of the staff, and drags the wire index where the knife encounters it in the staff.

I now proceed with the operation, the patient

being placed in the usual attitude, and chloroform having been administered.

You see I introduce the staff with ease, and touch the stone with it. I carry the handle well forward and to the left side. I now bring down the hinged-cutting director, and with precision apply its point to the integuments on the left of the anus, and nearly on a level with its anterior margin. It is thus directed somewhat obliquely upward and forward, behind the bulb of the urethra, toward the membranous portion. I now press the director firmly down, and it cuts its way into the hollow of the staff, between the bulb of the urethra and the crust of the penis. It is firmly fixed, and cannot change its place.

I now apply the back of the gorget knife, the handle of which has an angle with the blade, to the groove in the cutting director, and penetrate the hollow of the staff. I now press the body of the staff well to the right side, so as to make more room for the incision in the left perineum, and I cause the knife to glide along the spiral opening in the staff, directing its edge obliquely outward and downward. You see that it drags along with it the wire index, which causes no impediment. I have reached the extremity of the staff in the bladder. I now withdraw the knife, and, as I do so, dilate the outer portion of the wound.

You see me now introduce my finger before I withdraw the staff. I penetrate the bladder with it—the incision is sufficiently free. I now raise the cutting director, and withdraw the staff, retaining the index finger of the left hand in the wound.

Guided by the finger, I now introduce these forceps (comparatively small and short.) I seize the stone without difficulty, and am now attempting its extraction. I feel it break in the forceps, it being friable. I extract the principal portion. I feel often fragments with the finger. You see I have no difficulty in seizing them. I reach every portion of the interior of the bladder with the finger. I have now extracted the last fragment, and feel that the bladder is free. I shall not inject the bladder, because I explore it so freely with the finger, and feel that nothing remains. The operation is completed. You observe there is but little hemorrhage—none requiring any mechanical expedients to arrest it.

I shall introduce no canula, which I often use. I omit it in this case, because the opening is free and the perineum not deep, and because, in the recent case of Gov. Hicks, I found it to create irritation.

In Braithwaite's 'Retrospect,' Vol. 39, you will find a description and plate of an instrument, the invention of which is claimed by Mr. Corbett, of the Glasgow Royal Infirmary, and which is identical with mine, except that the bend in the staff is angular and not abruptly curved as mine is. This deviation only impairs the usefulness of the instrument.

I published an account of my instrument in 1831, and, from that time to the present, have employed it in presence of the medical classes of the University of Maryland.

The advantages which I claim for this instrument are, that it greatly simplifies and abbreviates the operation—that it gives unity and continuity to the incisions—that it obviates the danger of wounding the artery of the bulb, and the danger of the slipping of the knife or gorget from the groove of the staff.—*Maryland and Virginia Medical Journal, May* 1860, *p.* 345.

DISEASES OF THE SKIN.

78.—STATISTICAL ANALYSIS OF FORTY-TWO CASES OF RODENT ULCER.

By JONATHAN HUTCHINSON, Esq.

[This gentleman has published a valuable series of cases on this subject, which we have not space to give in detail. We will, however, give some of the most important results.]

The parts by far the most liable to be affected by the rodent ulcer are the eyelids and adjacent portions of cheek. Next to them the nose is its favourite site, and the third in the list are the cheeks. On one or other of these positions almost all the rodent ulcers occur. No case has yet been recorded in which this ulcer occurred in the integument of the extremities or on that of the trunk, if we except one case, in which the nipple was the part involved. It is a remarkable fact that in only one instance was the upper lip the primary seat, and that in no single case was the lower lip involved either primarily or by extension. This fact is of great importance, when it is borne in mind how frequently the lower lip is the seat of epithelial cancer. The single case in which, as just stated, the upper lip was affected, is not wholly exempt from doubt as to whether, indeed, it did not approach to the epithelial class of growths, since, unlike what occurs in a rodent ulcer, a lymphatic gland was enlarged.

Questions of Treatment.—The rodent ulcer is to be viewed, as far as our knowledge extends, as a local disease, which spreads by continuous growth. There is not a tittle of evidence in favour of the belief that it can be influenced by internal medication. The object which the surgeon must keep in view is to remove the whole of the diseased parts, and to procure a soft, supple cicatrix. If there be any tension on the cicatrix, the irritation thereby produced will be almost certain to induce a relapse of the disease. By means of deep escharotics, such as the chloride of zinc, complete destruction of the diseased parts may in many instances be ensured, and a very healthy and pliant cicatrix is usually obtained by this remedy. In many of the favourite

seats of rodent ulcer,—the eyelids to wit,—the use of escharotics is very inconvenient; and in these, free excision, followed by the transplantation of healthy skin from the forehead, cheek, or temple, is the best measure.

Aphorisms respecting the Rodent Ulcer.—1. That there occurs not infrequently on one or other part of the face a form of ulceration which is characterized by an indurated edge, and by a tendency to spread to adjacent structures, without regard to difference of tissue; which is very slow in its progress; does not cause much pain; does not induce cachexia, and is never followed by enlarged glands or deposits in the viscera.

2. Sections of the indurated edge of this ulcer (or of the portions of new growth which are sometimes produced about it) do not exhibit the cell-structures met with in epithelial or scirrhous cancer, but only those of organising fibrous tissue.

3. This ulcer differs from lupus exedens in that it never occurs in the young, and never gets well spontaneously, while lupus exedens but rarely begins after the age of thirty, and usually tends after the lapse of time to cicatrise spontaneously. The two, also, further differ, in that lupus has a tuberculated, inflamed border, without any degree of induration; while the edge of the ulcer in question presents an extremely indurated ridge, without tubercles, and comparatively free from inflammatory congestion.

4. The ulcer in question differs from cancer in that there is but seldom present any tendency to the production of new material, that it never causes the glands to enlarge nor induces morbid growths in the internal viscera.

5. Although it must be freely admitted that this disease is closely allied to cancer, and that in its inveteracy under treatment, and its tendency, if not removed, to spread deeply and extensively, it well deserves the designation of "locally malignant," yet it is inconvenient in practice to call it "cancer of the skin," since there are other forms of cutaneous cancer (the epithelial, scirrhous, melanotic, &c.) essentially different from it, and of a far higher degree of malignancy.

6. The term "a peculiar ulcer occurring in the eyelids," is too vague, and also involves an erroneous statement as to uniformity of location, an objection which, also, in addition to what has been stated above, applies to "cancer of the eyelids," since this ulcer is met with on many other parts besides the palpebræ.

7. To the designation of Rodent Ulcer given to this disease by Lebert, and adopted in this country by Paget (see Lectures on Surgical Pathology) no objection applies, excepting that it is more vague than desirable. Of those in use it is certainly the best, and should the disease become generally recognised by the profession under that name, the vagueness of its meaning will by custom soon cease.

8. The rodent ulcer is most commonly met with between the ages of 50 and 60, and is equally frequent in the two sexes.

9. It occurs but very rarely on any other region than the integument of the face, and is most common in the eyelids.

10. It is a singular and very significant fact that no case has yet been recorded in which the rodent ulcer attacked the lower lip, either primarily or by extension, while that part is well known to be a very frequent seat of epithelial cancer.

11. The *diagnosis* of rodent ulcer is usually easy. An ulcer with a hard sinuous edge, situated on some part of the skin of the upper two-thirds of the face, of several, or, perhaps, many years' duration, almost painless, and occurring in a middle-aged or elderly person, of fair health, and without enlarged glands—such a sore is almost certain to be of the rodent type.

12. The *prognosis* of rodent ulcer varies with the stage of the disease and the treatment it is intended to pursue. If left to itself it will slowly but surely advance both in extent and depth, and will probably destroy the patient's life in the course of from ten to twenty-five years, death being eventually produced by the exhaustion consequent on suppuration, hemorrhages, pain, &c., and very probably aggravated by inability to take sufficient food owing to the diseased state of the mouth. If the case be seen in an early stage while complete removal either by knife or escharotics is practicable, a favourable opinion may be given as to the probable non-return of the disease. The younger the patient the more rapid will be the course of the disease, and *vice versa;* and the younger the patient the more nearly is the disease allied to cancer, and the more likely to recur after removal.

13. The only *treatment* which the rodent ulcer admits of is local, and the best is that which obtains its freest removal with the least injury to the parts concerned. In some localities, and in some stages, escharotics, such as the chloride of zinc, may be advisable, but in most excision and transplantation of skin is the more certain and satisfactory.

14. A widely-diffused knowledge of the true pathology of rodent ulcer may be expected to result in considerable advantage to the sufferers from that disease, since it will encourage to the early and free adoption of local measures and to the employment of excision and transplantation even in some cases which, if considered cancerous, would certainly be beyond relief by surgical art.—*Medical Times and Gazette, Sept.* 29, 1860, *p.* 305.

79.—ON THE TREATMENT OF FAVUS.

By Dr. W. T. Gairdner, Physician to the Royal Infirmary of Edinburgh, Lecturer on Clinical Medicine and on Practice of Physic.

[In the case which forms the text of the following remarks, the head was at first covered with yellow crusts, of long standing; exactly four days afterwards, there was not a vestige of a crust to be seen, nor

even any broken surface, though the patches of absolute baldness and the stunted and diminished hairs in many places, showed clearly the deep hold the disease had taken. The change was entirely due to the successive poultices of linseed meal.]

After so much has been written about favus, and so many perfect cures have been recorded in periods varying from six weeks to several months, I am almost afraid to state my conviction, that the satisfactory result above mentioned, obtained in four days under linseed-meal poultices, was quite as much entitled to the name of a cure as any that I have yet seen or heard of either in nature or in the records of medicine. To speak of a cure of this disease, with opportunities of observation extending over less than a year or two, is, in my opinion, evidence of nothing else than the most entire ignorance of its habits. I do not, however, doubt the cure of favus. Soap and hot water, with abundant scrubbing, the hair being kept short, will commonly keep the yellow crusts indefinitely in abeyance; as will also, perhaps more thoroughly and effectually, the simplest oil inunction. There seems no reason, therefore, to believe (though hospital physicians can but seldom hope to witness the result) that these simple means, *long and perseveringly used*, will not effect the cure of a disease which owes its origin and perpetuation to nothing else than want of cleanliness.

Under ordinary circumstances. what takes place after an apparent cure of favus is this:—So long as the hair is kept shaved, and an alternation of oily applications with soap and water is maintained, the disease does not reappear; but on neglecting these precautions for a few weeks, yellow dots begin to crop up, and these rapidly extend so as to become distinct favus crusts, which in no long time, if uninterfered with, will cover the whole head. I have repeatedly kept cases under observation after the head had been completely cleared, in order to observe the first beginnings of the eruption after the suspension of treatment; and I have also employed a great variety of medicated ointments and lotions, including sulphurous acid, iodine and sulphur ointments, empyreumatic oils, mercurial ointments, and mixed medications of various kinds. After most of these, I have seen the disease reappear about as quickly as under the simpler treatment by oil and soap. If there is any of them in which I have faith more than another, it is in empyreumatic oils, as the juniper tar oil or the common pitch ointment. But the inveteracy of the disease evidently depends, not on the difficulty of removing its visible traces, but on the complete infiltration (so to speak) of the scalp with the sporules of the fungus in all old-standing cases; and no treatment will be of the slightest avail towards a radical cure that is not deliberately and carefully pursued until a complete new growth of scarf skin has been obtained, perfectly free from all traces of the noxious germs. This must, of course, be the work of a considerable time; just as it

is a work of time, and of unwearied attention to simple details, to rid a virgin soil of ragweeds and whins, or even of stones. No application of a specific can be expected to meet the one case, any more than the other.

One point, not always observed by those who have written on this subject, is, that favus is often, perhaps even in the majority of cases, implanted on the basis of a previous eruption; in other words, that the fungous crusts, or vegetable mould, are sown on a soil already the seat of impetigo, eczema, or some other variety of disease of the skin. Sometimes the original disease has died out when the favus first comes under treatment; at other times, it still persists and requires separate treatment. In the course of considerable and varied experience of true favus, however, I have not seen a single case that did not at once yield to local treatment, to the extent I have indicated above; and I am very far from believing that any constitutional disorder has to do with the production of the fungus, further than that favus and other diseases may arise simultaneously, under exposure to the same causes of filth, neglect, and hygienic errors of every kind, in every variety of bodily constitution.—*Edin. Med. Journal, May*, 1860, *p.* 1003.

80.—*Glycerine in Skin Diseases.*—This substance has been justly recommended in various affections of the skin, and especially in those attended with desquamation. In that troublesome affection *pityriasis capitis*, in which the hairs become dry and fall off, during the abundant epidemic exfoliation, undiluted glycerine may be applied with excellent and durable effect. In *pityriasis rubra* and *pityriasis simplex*, a mixture, composed of equal parts of oil of almonds and glycerine, and one-half of oxide of zinc, has proved very useful.—*Dublin Hospital Gazette, May* 15, 1860, *p.* 158.

81.—*Treatment of Eruptions around the Anus.* By Joseph Bell, Esq., Gateshead.—Occasionally we see obstinate cutaneous ulceration surrounding the anus in children. Considerable tumefaction attends it betimes, and deep fissures are occasionally seen. This disorder is probably herpetic, and almost always can be cured with yellow wash. The proportions being from one grain to one grain and a-half of hydrarg. bichlorid, to one ounce aq. calcis. The part is to be frequently bathed with it, and should the lotion produce pain it is to be diluted with water, and when at rest a little lint soaked in the lotion is to be applied and left on the part; deobstruents being at the same time administered.—*Med. Times and Gazette, July* 7, 1860, *p.* 23.

82.—*On the Therapeutical Methods of preventing Pitting of the Face in Confluent Small-Pox.* By Dr. Stokes, Dublin.—During the last five years Dr. Stokes has employed gutta-percha and collodion

in a considerable number of cases of confluent small-pox, for the purpose of preventing pitting of the face. In most of the cases the crust came off in large flakes or patches, composed of the dried exudations and the covering material, leaving the skin uninjured. This kind of treatment was most successful in cases of a typhoid character, but appeared to be not so well adapted to those presenting a more sthenic type. Dr. Stokes considers that the application of poultices over the face is the surest method of preventing disfigurement in small-pox. Their use should be commenced at the earliest period, and continued to an advanced stage of the disease. In most cases they may be applied even over the nose, so as to cover the nostrils. This plan should fulfil three important indications of treatment—namely, to exclude air, to moderate the local irritation, and to keep the parts in a permanently moist state, so as to prevent the drying and hardening of the scabs. The best poultice is formed of linseed meal, which should be spread on a soft material, such as French wadding, and covered with gutta-percha paper or oiled silk. The conclusions to which Dr. Stokes arrives are the following: 1. That the chances of marking are much greater in the sthenic or inflammatory than in the asthenic or typhoid confluent small-pox. 2. That, considering the change in the character of disease observed during late years, we may explain the greater frequency of marking in former times. 3. That, in the typhoid forms of the disease the treatment of the surface by an artificial covering, such as gutta percha or glycerine, will often prove satisfactory. 4. That in the more active or non-typhoid forms the use of constant poulticing, and of every other method which will lessen local inflammation, seems to be the best mode of preventing disfigurement of the face.—*British and Foreign Medico-Chirurgical Review, July* 1860, *p.* 248.

DISEASES OF THE EYE AND EAR.

83.—ON CORELYSIS.

By J. F. Streatfeild, Esq., Editor of the Ophthalmic Hospital Reports.

[A short notice appeared in the journals about three years ago, upon the subject of Corelysis. Mr. Streatfeild has now matured the operation, and we insert the most practical parts of his paper, which, in the original, would occupy too much space for this work. Mr. Streatfeild has been perfectly successful in freeing the iris whenever it is adherent at its pupillary margin, whether to the lens or cornea, or to a false membrane in the pupil itself. He now names his operation *Corelysis* (meaning Pupil freeing, from Κόρη and λύσις.)

My paper will throughout appear to have a double bearing, as it refers to the two cases in which either the pupil is bound to the lens

or cornea, or to a false membrane in its area, but they are alike and are here always considered together, for in either of them it is required to *free the pupil.*

As to the *prevention* of pupillary adhesions I have no other suggestion but that the old belief in belladonna and mercury should be cherished, that the former should never be neglected at any time in the treatment of iritis, and the latter should be generally administered in very frequent small doses continued for some time after the disappearance of inflammation, with good food or perhaps some iron or other tonic, as well as the specific medicine. At the conclusion of the treatment of a case of iritis the absence of any adhesion should be ascertained, if the iris is not perfectly active, it may be because of a single band between it and the lens; and if at this time the atropine will not cause its separation, it is certainly, in my opinion, to be divided.

Until I commenced freeing the pupil in so many of the cases in which it is bound, I believe adhesions were disregarded, as far as any surgical or other remedial attempt is concerned, in any less serious cases than those of virtual blindness, that is to say, that unless there was very extensive posterior adhesion, the pupil was closed, or so in effect, the adhesions were allowed as of course. If belladonna was applied it was for the purpose of discovering an interval of the adhesions for the contemplated artificial pupil, whereby the iris was to be torn, cut, or excised, the adhesions themselves being left, and a confessedly ill-placed pupil made by one of the old methods. Belladonna will in fact seldom effect the severance of pupil-adhesions, when a patient applies because of them, and not for the efficient inflammation, then no longer existing; it may do so, it should be tried, and the discovery of an interval of non-adhesion of the pupil is to me of as great importance for the operation on the membrane which obstructs the pupil as it is for making an artificial pupil, in these cases the most urgently requiring an operation, or in any case for corelysis. Ths new method can, therefore, be only compared with corectenia, corotomia, corectomia, and some others, all of which are really operations on the iris, not like corelysis on that which confines the pupil, or occupies its place. These adhesions then have been permitted, but if all useful vision is lost in any case, the iris has been cut through or a part of it removed and an artificial pupil, necessarily out of the axis of vision, has been formed; these pupils are certainly not the best that could be imagined, and the operations on the iris, supposing them unnecessary in these cases, are also, it now seems to me, too severe, but for those of them in which the lens capsule certainly cannot be cleared in the area of the pupil. If, for the sake of conservative surgery, the iris should not even always be operated on for occlusion of the pupil, there has been introduced an heroic operation, which I believe to be quite unjustifiable, for the deliberate extraction of the transparent lens in these cases; if, by corelysis, a cataract is dis-

covered, it can be removed by way of the restored pupil, but I believe the lens is seldom opaque in cases of occlusion, and I know that although adhesions may exist between the false membrane occluding the pupil, and the lens-capsule, with which it is in apposition, they are probably only partial and much weaker than the capsule itself, at any rate in the cases presenting the external appearances to which I shall allude.

When corelysis, instead of doing nothing operative or making an artificial pupil, seems to be the alternative, it is because the ordinary medicinal agents used to prevent the adhesions or for their detachment, have been ineffectual; mercury has been tried, and no ciliary redness or other signs of latent inflammation exist, still the tentative use of belladonna may be made in any case, although it has probably been tried, and that when the adhesions more recently established would be much more likely to give way. I do not know for certain how recent must be the adhesions, which, by the use of belladonna, will give way, which will be thus separated, or on what other conditions of treatment, or the state of the patient's health, this will depend; old bands will be extended by its use, if they are narrow and isolated, and recent adhesions, *only, as far as I know*, may entirely disappear. (It is a common mistake to suppose that of course adhesions found to exist with iritis, have resulted from the present inflammation of the iris). When the adhesions are to a false membrane in the area of the pupil, I believe this membrane, however slight it may be, can only be dispersed by corelysis; no medicinal agent will remove it, if it exists after the inflammation has ended.

Atropine—or, applied to the skin, belladonna rubbed down with glycerine—should be used without delay in the cases, whether recent or determined, in which the pupil may be or has been involved; if employed when the pupil can dilate fully, the iris is so removed from the lens that it can hardly become bound to it by the opposite effect of the iritis, and then lymph having been effused between them, the mydriatic effect will, perhaps, be enough to cause their separation, and if not, the patient's vision is thereby probably improved for a time, and it is altogether essential to the surgeon for an exact knowledge of the state of the case. It is most important that the agent should be repeatedly employed; a solution of a quarter of a grain of sulphate of atropia to an ounce of water, will produce full dilatation in a healthy eye (but, in iritis, past or present, the effect is varied in the different cases), a strong atropine solution cannot cause more dilatation than this, but by it the effect is brought about more rapidly, it also probably lasts longer, and, when the iris is inflamed, perhaps it is the best to be employed (a shade worn by the patient may promote the dilatation or its continuance); and if these means have been ineffectual they should be persevered in, whilst inflammation exists, because the treatment otherwise adopted may give an opportunity of dilatation in time, but if there is no inflammation they may be hope-

lessly discontinued. That posterior adhesions not unfrequently become detached is proved by the ophthalmoscopic examinations, in which, with a pupil properly dilated, their remains are seen often as a circle of irregular pigmented dots, or one or two opposite the margin of the pupil when it has been contracted; these, and, after corelysis, the like dots are compatible with very good vision.

The basis of the new method of operating is expressed in the title first applied to it of "reinstating the pupil," it is founded on the wish to restore the normal state of the parts, to imitate it better than by artificial pupil, or to improve the vision of those patients with bands of adhesion after iritis, in whose cases they have been left, if not with impaired vision, with probability of a recurrence of the inflammation which had caused them. For this purpose the pupil must be freed, and, if it was practicable, could be only found by the attempt in the different cases in which it is bound. Could the bands of adhesion confining the pupil be broken through by operation without injury of any of the parts concerned; and could the pupil be released from any of the membranes, which after iritis have blocked it up, by their being dispersed by operation, and the natural aperture of the pupil be thus made free again to dilate and contract in any degree? This position I have practically endeavoured to take affirmatively, and now I hold it so. That *synechiæ* are very prejudicial to the future integrity of the eye is my own conviction, but this can add little to the testimony of those who have so much experienced authority; of posterior adhesion, Von Graefe has said "*that the principal cause of the recurrence of iritis is the existence of synechiæ, especially when broad and inextensible.*" Adhesions not only may induce a second iritis, but when the iris is first inflamed they may protract the recovery indefinitely: a single point of adhesion may do this, as in a case of mine in May, 1859, in which, with mild mercurials a syphilitic iritis had ended in resolution, with no ill result but a single point of adhesion to the lens, all redness of the globe had therefore disappeared, but opposite this one band it remained exactly there, and there only for a long time afterwards.

Of the advantages of corelysis, whenever we may consider its possibility instead of making an artificial pupil, I believe the most important to be the optical superiority of a central active pupil, the appearance is also an advantage dependent, in fact, on much the same qualities as those for vision; these better optical qualities which almost alone need be considered being the natural position of the reinstated pupil, and the better recovery of its normal adjustment in size; this power of dilating and contracting being often perfect in cases in which the bands of adhesion have been few and narrow. Of the operation, I must say, in anticipation of the objections of those who believe the lens-capsule is so very easily torn, that in about fifty cases cataract has only once followed, when I have known for certain beforehand that the lens was clear; this case was one of the earliest on which I

have operated, and the first spatula-hook I had had made and then used, was not perfectly rounded or well polished; of other ill results of the operation I say nothing, for I believe, if the cases promise well for the operation and all care is taken in its performance, there will be none, or that none have resulted that do not more frequently occur when the iris is operated on for an artificial pupil: of this I shall speak again. When I call the operation trifling I do not mean that it is so to the operator, for him, I believe, it requires much consideration beforehand to determine upon its adoption, an examination by daylight and with atropine and the opthalmoscope, the latter to decide if any space seeming clear is really unobstructed, or if occlusion seeming complete, really has no space left for the instrument to be admitted. Besides this perfect knowledge of the state of the parts, and so of what can be done, it requires the closest attention on the part of the surgeon, during the operation, to the exact place of the lens, and above all things that the end of the instrument introduced through the membrane or beneath the bands of adhesion should be then at once carried away from the lens and kept always forward. Lastly, of the advantages of corelysis, I must remark, that when other apertures of the body are obstructed by disease, we do not think of making a false passage, or a counter-opening if it can be avoided; and if corelysis is impracticable or unsuccessful, still as before, we can make an *artificial* pupil.

Corelysis should certainly not be attempted until the ciliary redness and other signs of inflammation are past; I do not propose it as a cure for iritis, though it may divert its return. The inquiry should be always first made before operating, if the lens is transparent or not; this, in the case of an occluded pupil, can only be learnt by the probabilities connected with the amount of vision left, but, of course, if the pupil is only limited by bands of adhesion, atropine and the ophthalmoscope will go to prove an uncertain case. If the depth of the eye can be examined, it is well for a favourable conclusion of the case to know also that there is no cognizable vitreous disease or white patches in the choroid, &c. If, with adhesions of the pupil to the lens, cornea, or a false membrane, I have known the lens to be opaque (corelysis, *for its own sake*, is out of the question), I have proceeded to operate for the cataract, then always using atropine; if by *extraction*, the corneal section is made very readily, and without need of the chief ordinary precautions, then, and especially at its completion, necessary: a membrane filling the pupil may be opened out with one or two needles to make a way for the lens, as the common practice has been; or if separate points of adhesion of the pupillary margin exist, I have preferred my spatula-hook for their detachment. If it is to be done by *solution:* in the case of a false membrane, I have used the fine needle or needles to open out the membrane (without enlarging the pupil), and then to the cataract itself; or in the case of bands partially confining the pupil, I have introduced the needle where they

can be most conveniently reached, and with it broken them through as a preliminary step: two needles may in such a case be well used if bands of adhesion at opposite sides of the pupil are to be reached. The needle operation for the cataract with iris-adhesions, and subsequent *linear-extraction*, may be well adopted if, as is often the case, the patient is young, and the inflammation has not been allowed to proceed to more serious injury than the limiting of the pupil. The patients are not so old as most of those who require an operation for cataract, so that "division" of the lens will have this advantage, and linear-extraction afterwards, if the pupil is large enough to allow of it, will curtail the task that by the sound eye may be better performed. This "extraction through a closed pupil" by a small or large section, however, is hardly connected with the subject of corelysis, but as a supplement to the latter operation when it has revealed a cataract not before known or believed to exist; indeed it is especially applicable to some cases in which corelysis, *for itself*, as I have mentioned, should not be attempted, those in which a cataract is known to exist, or the occluding membrane is quite opaque and far back in the contracted pupil, &c.; and corelysis, as I wish it understood, is for those cases in which the disease of the pupil is the only serious morbid affection that exists.

The instruments necessary for corelysis are, besides the spring-speculum and forceps with which to hold the eye, a broad needle, my spatula-hook, and perhaps canula, scissors, and forceps. The broad needle should be a small one, only to make an opening large enough, without cutting, to admit the other instruments (this may be proved beforehand with the aperture made by it in a piece of leather), so as to retain the aqueous humour as much as possible. The spatula-hook is figured and described at page 8 of the first volume of the 'Ophthalmic Hospital Reports:' the thickness of the spatula part, "the terminal quarter of an inch," should be equal, and not much reduced so that it may be very blunt (the instrument, especially at its extremity, and the hook, should be highly polished); the only difference I have made from the instrument figured is that the one I now use is rather less altogether in its width and every way; the spatula part should, moreover, be wider than the diameter of the round part of the instrument (made so next the handle for facility of turning it "in the corneal wound upwards or downwards"), that when it is in the anterior chamber, the aqueous humour may be better retained. The canula instrument should be the smallest, and well polished: the scissors should have blunt ends, and the *outer* side of the blade used should be kept next the lens during the operation.

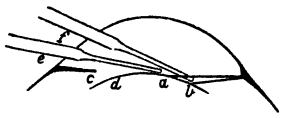
Of the dangers of corelysis in anticipation of the operation, especially of causing cataract, I have already spoken, in comparing the reinstated pupil with iris-operations. I do not know how severe an injury of the lens-capsule is necessary to produce a cataract, but I am sure with instruments well made for the purpose I frequently touch

it without any ill result; but, as I have said, I always keep the end of the instrument rather forward, that is to say, against the posterior surface of the iris or pupillary exudation, in detaching bands or a false membrane, that I may touch the lens as lightly as possible. I should not be so careful in this matter but for the difficulty of perceiving the minute distance between the iris and the lens, and of appreciating the slight changes of place which would bring the instrument upon the lens; so that, during corelysis, I constantly bear in mind to keep the end of my instrument away from the lens. I have, at the present time, a case at Moorfields in which the lens would, according to the common notion, have been expected to be changed from its transparency.

My method of operating is the following: atropine has been applied to the eye; the patient is recumbent, and I stand behind him. I have chosen a broad needle which is suitable to the spatula-hook, that is, as I have explained, a little wider than it; the patient has probably taken chloroform. I secure with toothed forceps, in the right or left hand as it may be most convenient, a fold of the conjunctiva close to the corneal margin on the side opposite to that at which the puncture is to be made, thus I draw the eye sufficiently in the desired direction and steady it. (It is best to secure a considerable fold of the conjunctiva close to the cornea, and to have a deep hold of it that it shall not give way; sometimes it is convenient not only to draw the eye with the forceps in an opposite direction to the proposed corneal puncture, but at the same time to rotate the globe somewhat on its antero-posterior axis: this may be done either way by having the conjunctival fold a little to one or other side of the direction in which the globe is to be drawn.) It is impossible, in the variety of cases in which corelysis is applicable, to say generally which way the globe is to be held, because, for instance, if an adhesion of the pupil is most conveniently to be reached from above it may be detached without any rotation of the globe, if it is so prominent that the brow will not interfere; or in another case the reflection from the cornea of the light of the window opposite the patient may make some rotation of the globe, when it is held with the forceps, more convenient than a change of place on the part of the operator. It is also impossible to say where the puncture should be made in the cornea, even at what distance it should be from the margin: this is, in each case, very important, for, when made, the corneal opening is a fixed point from which alone the operation has to be carried on to its completion. I have not generally made it very near to the margin (perhaps at a line from it), but my rule will be found by considering first, that a (posterior) adhesion should be always detached as close to the lens as possible; second and third (in this special instance), is the band corresponding to the position of the pupil when contracted or dilated? and so at what part is it of the convexity of the lens? of which the natural curve is to be considered (Fig. 1), as it should not

be pressed upon by the instrument as it passes over it to reach the band at its point of attachment, the severance of which alone is to be attained; on the other hand, it can never be required to make the puncture far from the corneal margin, or the end of the straight instrument might come upon the lens, which it would be much more likely to injure than the flat side of the spatula would.

FIG. 1.



a. A posterior adhesion of the *contracted* pupil.
b. The same, of the pupil when it has been less contracted.
c. The normal iris.
d. The lens.
e. The spatula-hook introduced througd the cornea in the best position to detach an adhesion at *a*.
f. The same, to detach an adhesion at *b*.
The figure is intended to show that to detach adhesion *b*, the spatula-hook must not be introduced so near to the margin of the cornea (seen in section) as at *e*, or the adhesion cannot be reached *at the point of its attachment without pressing upon the lens.*

The easiest case of pupil-adhesion to be detached is when a single band connects the pupil with the lens, then the corneal puncture may be made most conveniently, perhaps (as in Fig. 2, *M. n.*) not exactly on the opposite side to that at which it is, or at a quarter of the circumference of the cornea, but between the two, that the spatula-hook may be advantageously used as a spatula or as a hook; as a spatula, that is to say, for its blunt edge, the instrument is most generally useful, and perhaps most safely or easily used, as the stretching of the iris that may occur, is thus more distributed than by collecting some part of the width of the iris, to which the band is adherent, in the hook to be severed at once. With the instrument entered in the direction I have described, the band, if it does not detach before the blunt edge, will pass downwards with the sloping direction of the instrument, and be arrested by the hook, with which it can be then broken through. If two such bands exist at opposite points of the pupil, the spatula-hook will be most conveniently used when entered from a part of the cornea, towards its margin, midway between them (see Fig. 2, *N. m.*), and then, if the hook of the spatula is required for their detachment, the instrument must be semi-rotated when the first one is separated, before the instrument is applied to the second band. When the instrument is thus rotated in the anterior chamber (the aqueous humour should not have escaped and) the end of it should be brought forward towards the central part of the cornea, that as the blunt edge instead of the flat side of the instrument passes the lens, it may only touch it lightly, if at all. If many bands exist the instrument is introduced to most advantage from the side at which there is

the greatest interval for its admission (see Fig. 2, *N. n.*), the division, of pupil attachments being always attainable excepting on the side at which the instrument is being used (Fig. 2, *M. m.*). The broad needle to make the opening in the cornea, is directed towards the pupil and is introduced with a thrust, without cutting, as far as the widest

FIG. 2.

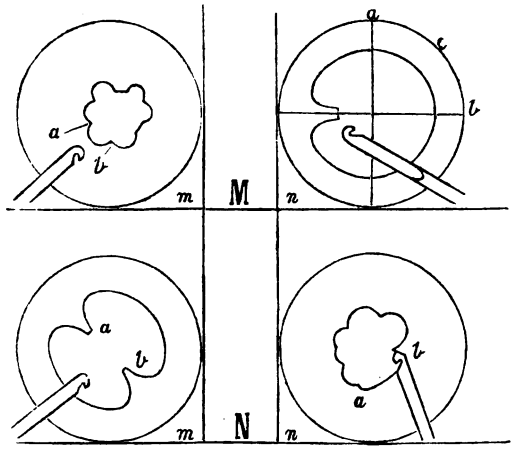


Fig. 2. The four diagrams show (magnified) various posterior adhesions of the pupils, all under the influence of atropine.

M. m. The six points of attachment, nearly equi-distant, are so much the more difficult; the two, *a* and *b*, cannot be easily detached with the spatula-hook introduced through the corneal margin thus most conveniently for the others.

M. n. The instrument, to reach the point of adhesion, is best entered where it is (or at *c*) and not at *a* or at *b*.

N. m. The spatula hook should be brought up to the two opposite adhesions (*a* and *b*) from a point midway between them.

N. n. With such various adhesions the instrument is used to most advantage at the widest interval (between *a* and *b*) and can be passed (as it is shown at *b*) even behind those on the same side as the corneal opening.

part of its blade, to which its size in relation to the spatula-hook has been calculated beforehand; it is then quickly withdrawn, and the spatula-hook as quickly passed in by the opening that is made, the hook being on the upper or lower edge as may be required, the end of the instrument is brought up to the adhesion and passed under the margin of the pupil (as at *b*, in Fig. 2, *N. n.*) close to it and far enough beneath the iris; the end of the spatula beingat the same time kept in apposition with it, the instrument is then brought towards the pupil up to the adhesion, not pressing the iris forwards or the lens

backwards, but just between them, that the attachment may be released, as it generally will be very readily at its place, without tearing the iris or compressing the lens; in all this it must be remembered that the lens is not felt in its position, the direction of the spatula-hook is a matter of calculation throughout, the course it is to take has been determined and is well known; and the iris or its pupillary adhesions will offer no appreciable resistance. If all the cautions I have given respecting the lens, appear contrary to what I have said of the unnecessary fear commonly entertained of its being wounded, I can only say, that by taking such care I have had the success which induces me so confidently to ask attention to the new proceeding, and I do not know that cataract would have resulted from less caretaking. The withdrawal of the spatula-hook should be, of course, with the flat part in the direction of the length of the corneal puncture, and it is effected without any chance of the hook being caught in the cornea, if the edge of the spatula, *without the hook*, is kept lightly pressed into the corresponding extremity of the linear wound.

The most important cases for corelysis remain to be considered. I have known vision more impaired with bands confining the pupil to the lens, than with a false membrane, leaving to all appearance in the pupil no free space whatever; the case of simple bands of posterior adhesion with very imperfect vision, may be possibly explained by various complications of the iritis, which has caused the adhesions limiting, but not blocking up the pupil; and the other case in which a complete false membrane filling the pupil exists with fair vision, by its being only a very fine network, thinnest at its centre, in the pupillary area. These false membranes generally have been recognized as surgical (artificial pupil) cases, but only to be operated on when serious loss of vision is the patient's complaint. The occluded pupils which I am about to consider, in an operative relation, are also most important, inasmuch as they are most difficult, and as they are certainly irremediable but by operation. Here I will premise that it seems to depend very much on the diagnosis of the false membrane, if corelysis can effect a cure when the pupil is filled: the cases suitable for the operation, are not those in which a perfectly opaque patch in the pupil is seen with an iris, more or less standing forwards around it, the membrane is not of a dead white, but gray by daylight, or half illuminated by the transmitted light of the ophthalmoscope, and it is little behind the iris, which does not fall back towards it, or is not bulged around it, and is not discoloured: the condition I allude to, in which the iris is in its natural place, and the membrane filling the pupil is about on a level with it (which with its partial transparency are, I believe, the indications for corelysis in cases of occlusion), is probably to be explained by two circumstances in which it differs from the opaque membranes with prominent iris, the closed communication between the anterior and posterior cham-

bers not being altogether impervious to the aqueous humour, and the membrane not being firmly attached behind to the lens-capsule, so that there, as it is not closely adherent, some fluid transition is allowed. Indications favourable to the operation, are the evident contraction of the fibres of the iris when the eye is exposed to a strong light, a large anterior chamber and the (occluded) pupil not very small, improvement of vision, so as to read large type, by the use of atropine, &c. My best results have been, I think, in cases of young persons who have been judiciously treated for a first attack of syphilitic iritis, and to whom some mercury has been already given. If the false membrane seems altogether to occupy the pupil, it must be examined with atropine and the ophthalmoscope, for the discovery of any possible gap in it, by which the spatula-hook may be introduced, a chink even smaller than the end of the instrument may be made to admit it, and the membrane is torn through with the hook of the spatula, or divided with the canula scissors. If the hook of the spatula is to be used, the instrument must be introduced through the cornea, on the *opposite* side to that at which the opening is in the membrane, and in this way I have generally concluded the operation at once: the canula-scissors can only be used when introduced from the *same* side of the cornea as the opening in the membrane, and probably, only with safety after the spatula has been passed between the membrane and the lens; this method, therefore, I have not followed since the case of Sharp, I have referred to. When no space at all is left in the occluding membrane, the plan I have adopted is to introduce the broad needle through the cornea, in any direction which may appear to be lengthwise of the membrane, then to advance the needle across the pupil to the opposite side of the membrane, and transfix it just within the pupil close to the iris, but carefully not to cause bleeding from it, raising the membrane from the lens with the needle as soon as the point has pierced it, and advancing the needle a little to make the opening larger, whereby there is no greater fear of wounding any part, as the needle has been passed *across* the pupil, and the convexity of the lens, and beneath the iris. Because of the spherical prominence of the lens which is close behind the pupil, it is clearly impossible with safety to pierce the membrane with the needle on the same side as that by which it is passed through the cornea, but I do not know whether it would be safe in any case, and the pupil could be cleared by passing the canula scissors at once through the cornea, and the blunt point through the crevice, *when there is one*, in an occluding membrane, without previously using the spatula to lift the membrane and separate it from the lens, by all which the aqueous humour is so much let escape, that as in Sharp's case, the scissors could only be adopted at a subsequent operation, which, of course, it is very desirable to avoid. I passed the spatula-hook once through the membrane and close behind it, to separate any filaments that might connect it with the lens, but I did not venture, at that time,

to use the scissors, because of the diminution of the anterior chamber which had then resulted. I am inclined to the adoption of my early plan, of passing the spatula *between* the lens and the membrane, *before* the latter is operated on, in every case.

As to the after-treatment of a case in which the pupil has been freed, I have no special knowledge; I have had atropine applied after the operation night and morning, the eyelids closed, to insure rest to the eye, perhaps with a bandage and cold water at first, with a folded rag constantly until any ciliary redness has faded, or it is no longer agreeable to the patient. I have never known any more extensive inflammation than that of the iris itself, follow corelysis: in several cases, by this consequent iritis, the pupil has again become occluded or bound to the lens by adhesions, and then no benefit has resulted: in two or three, the pupil has been more obstructed than it was before the operation; and if it is not, at once, subject to treatment, this traumatic iritis is fatal to the operation and altogether hopeless: leeches and other antiphlogistic treatment are the means I have employed for its reduction.

In operations generally on old remains of lens-capsule, &c., in the pupillary area, either long after operation for cataract or wound of the lens, I have learnt to fear especially two consequences that are not uncommon or inconsiderable; either the protrusion of the vitreous humour into the pupil, through the opening made, or chronic inflammation in the ciliary region, perhaps consequent upon or coexistent with the former, or, as I have thought, by dragging upon the suspensory ligament. Both of these ill-results may occur, the former on account of the transparency of the vitreous humour is often not known to have happened, and the latter is only manifested some time after the operation. Once lately I discovered that the former had taken place by the bleeding which occurred from (?) the ciliary processes: the blood filled the empty aqueous chamber except in the centre, where the vitreous had come forward and was pressed against the back of the cornea (I had torn open, with two needles, some remains of capsule, left in the pupillary area, after extraction of cataract), the blood surrounded the vitreous as a red ring before the iris, and through the centre of the cornea the clear vitreous humour and the pupil "opened out," the patient saw at that time very well. To obviate this projection of the vitreous humour into the aqueous chamber, which some have thought to be actually advantageous, when a false membrane (lens-capsule, &c.) is rent, as in the case I have just mentioned, the needles should not be passed farther than is just necessary through the membrane, or it should be divided with canula-scissors; it should not be lacerated very extensively, or repeatedly, and in the cases also of old traumatic cataract (remains of lens and lens-capsule) once operated on, it is best to be satisfied with a fair result for vision although some portions remain in the pupil

which might be removed; because these portions of lens-capsule, &c., after extraction or wound of the lens are *held in the ciliary region, by means of the (suspensory) ligament*, which has be perhaps confirmed and toughened by the former operations, and in their removal the vitreous humour is always likely to come forward, and perhaps also irritation of the ciliary body is induced. Considering this to be so, and the ill-results there have been, I devised the method of tearing through the ligament *in detail, before* attempting the removal of the obstructing lens-capsule, &c., it is obvious that if it is seized at once by the forceps, as it has been usual to do in these cases, the instrument has one-half, more or less, of the suspensory ligament opposed at

FIG. 3.

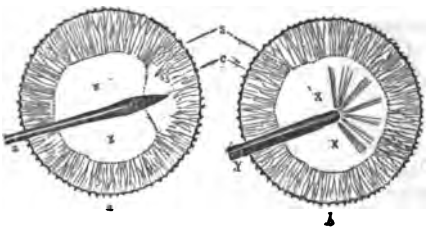


Fig. 3. *c*, ciliary process; *s*, suspensory ligament; *x x*, *x x*, shrunken lens-capsule, remains of lens, &c.

a, shows the manner of separating with the needle (*s*) the lens-capsule at its margin, in detail.

b, shows how advantageously the canula-forceps (*y*), are *opposed* by the retaining influence of the suspensory ligament, in such a case as the foregoing.

once to its traction, and acting very favourably *against* the removal (diagram, *b*, Fig. 3,) thus in fact, it is well adapted to support the healthy lens, and as I have said, it is, in an abnormal state, probably strengthened; however, with force applied at some one part of the ligament it is easily there torn through and the needle-point will effect this at various parts, and so entirely separate the lens-capsule, &c., and leave it to be removed with the canula-forceps, with little disturbance of the vitreous humour and without irritation of the ciliary body. In other surgical operations the *gradual* separation of parts to be removed is the rule whenever it is applicable, and in ophthalmic surgery it is often to be followed; the iris itself must not be dragged upon to a great extent at once; and so, in corelysis, as I have said, I have always avoided its ciliary separation (iridodialysis), or any laceration of its substance in detaching the strongest adhesions of the pupil. *by taking them in detail.—Ophthalmic Hosp. Reports, No.* 11, *p.* 309.

84.—ON IRIDECTOMY IN GLAUCOMA.

By WILLIAM BOWMAN, Esq., F.R.S., Surgeon to King's College Hospital, and to the Royal London Ophthalmic Hospital, Moorfields.

[In the 'Dublin Quarterly Journal of Medical Science,' a review lately appeared intended to discredit the treatment of glaucoma by iridectomy, as advocated by Von Graefe. Mr. Bowman was the first English surgeon who applied this treatment, and being convinced of its importance, has since constantly advocated its employment. The term 'Glaucoma,' as now understood, embraces much more than the etymology would import. Mr. Bowman says:]

The treatment by iridectomy has been no hap-hazard guess, but a rational conclusion gradually worked out. The dominant idea guiding the mind to the appropriate remedy has been that of the existence of augmented intra-ocular pressure, as the main characteristic of the glaucomatous process. In fact, whatever the essential nature of that process, its concomitant seems to be a marked tendency to hardness or tension of the eyeball: and thereupon arise the most serious secondary effects,—paralysis of the retina, oftentimes interrupted circulation, congestions, inflammatory attacks, with their various consequences,—ending sooner or later, and with more or less intermissions, in total loss of sight and a spoiling of all the tissues of the eyeball. The general states met with in practice and falling under the general heads of amaurosis with excavation of the optic nerve, chronic, subacute, and acute glaucoma, all seem to be allied to one another, and may be termed "glaucomatous diseases." Some are very slow in their progress, and the loss of power in the retina very gradual; there is no inflammation or even congestion: in others there is congestion, slight or considerable, intermittent or continuous. In others, again, acute and intense inflammation arises. A glaucomatous state may also come on in the course of other diseases, choroidal, retinal, or cataractous. To relieve intra-ocular pressure seems to be the prime indication in all, and it is rational to suppose that it will be the more effectual, the less deterioration of structure the retina has previously suffered. To relieve this pressure is to disarm the glaucomatous state of its chief peril, and apparently to restore the eye to the influence of the reparative powers belonging to it as a living organ. The tension once relieved, and so long as it remains so, the circulation tends to its natural equilibrium, and the retina which has been compressed recovers itself more or less, or ceases to degenerate, in a degree usually corresponding inversely with the intensity and duration of the preceding pressure.

It is certain that this critic would never have attacked the new practice had he really read Graefe's memoirs, or believed the cures reported by others, to be genuine. He says, "Graefe's doctrines, as published by the Sydenham Society, are plausible, but in a practical

science of this nature we require something more than doctrines; cases must be given, and Graefe was wiser than his countryman * of the London Ophthalmic Hospital, and kept his cat in his bag." Will it be believed that the reviewer can have read the memoirs of Von Graefe which he criticises, which are enriched with numerous cases, fully detailed and epitomised, and constantly referred to, illustrating all the main doctrines advanced? As for the experience acquired in England, I shall at present speak only for myself; and I can assure the reviewer, in the most decided terms, of the reality of the influence of this operation in relieving, even permanently, the unnatural tension of glaucomatous eyes, and of its effect in arresting the glaucomatous process, and often in restoring sight in a marvellous manner. My own cases in the hospital and in private have been numerous, and bear out the above general statement. This it is that makes me so anxious that the profession should be rightly informed as to the signs of glaucoma in its relievable stages, and should be ready to sanction the timely application of the only known real remedy. I can hardly conceive a greater comfort in practice than to be able, by this operation, to rescue sight thus imperilled, and to escape the disheartening task of treating a disease, evidently tending to blindness, by the old, ineffective remedies. The reviewer may class the eminent author of so great a boon with "Hahnemann, Preissnitz, and De Leeuw," but he may depend upon it he will be powerless to "arrest the spread of the 'glaucoma epidemic' in England." I could, indeed, heartily wish that when he has further examined into the evidence, and dispassionately perused what he criticises, he will make trial of the operation for himself, in which case I doubt not he will be satisfied with his results.

In speaking in these terms of iridectomy in glaucoma, I must carefully guard myself from being supposed to uphold it as a sovereign remedy equally valuable in all forms and stages of the disease, and under all its various complications. At present the operation is being extensively tried in cases where augmented intra-ocular pressure evidently exists; and the proved tendency of the operation to relieve this injurious tension, the certain cause of secondary mischief, makes it our duty to employ it, when the patient is otherwise apparently drifting into inevitable blindness. It may be our misfortune, and that of our patients, that our experience is as yet less complete than a few more years will make it; but meanwhile we can only act on the light we possess, and store up the results for the future benefit of others.

While the modern doctrine and treatment of glaucomatous affections is so new, it is to be expected that errors in the diagnosis may occur even among well-instructed practitioners; and the operation is

* Referring to the report of cases of operations for Glaucoma at the Moorfields Hospital, given in the 'Ophthalmic Hospital Reports,' by my friend Dr. Bader, who is quite able to defend himself.

doubtless in some danger of being brought into discredit by being undertaken in cases to which it is not rationally applicable, or by being defectively executed. I do not see how this can be avoided; it is the necessary fate of all novel proposals, and this one must face the ordeal. It is the part of intelligent men to make allowance for these incidents of human progress, rather than to allow them to prejudice the march of truth.

In the present state of our knowledge it may be right to try the operation experimentally in some cases to which more extended experience may show that it is not usually applicable. Experience hitherto shows that it is most valuable when performed *early in acute cases*, in which turbid effusions exist, the absorption of which goes on rapidly when the tension is relieved. Temporising measures in such cases, such as bleeding, mercury, colchicum, and even the even simple puncture of the eye, whether of the aqueous or vitreous regions, are not to be relied on, and occasion dangerous delay. It is also most useful if performed in the subacute form, when the visual field is not as yet seriously contracted, but where the symptoms are steadily advancing. If postponed here till the sight becomes greatly impaired by gradual alteration of the tissues of the retina, and of the optic nerve-entrance, the ultimate advantage is smaller, though usually lasting. In many instances we are called on to treat patients already almost blind from the progress of the disease under one or other of its various forms. Here it has often been the means of sparing them a little sight, sometimes permanently, at others not. I have also tried the operation in some examples of blind and tense glaucomatous globes, the seat of excessive or wearisome pain, and on several occasions have been well satisfied with the result. How intractable such cases are apt to be under the usual palliatives is well known.

As for the operation itself, a large experience has convinced me that, though usually a simple proceeding, not liable to accidents, it is occasionally one of the most delicate and critical in its nicety of all the operations on the eye. The anterior chamber has to be entered at its extreme rim, where the sclerotica overlaps the cornea, close in front of the iris, which often bulges much, and which must on no account be pricked, lest the vitreous humour or the lens, immediately behind, should be wounded; in either case a most serious complication. The bulging of the lens, in many cases, and the dilatation of the pupil, add to the risk of injury to the lens. A little blood, too, escaping into the anterior chamber, either before or after the excision of the iris, may obscure the parts from view, and add to the chance of involving the vitreous humour or lens. It is obvious that these difficulties and contingencies ought to be acknowledged and recognised in order that they may be guarded against. In very few instances indeed can they lead to accidents in skilful hands. When blood occupies the anterior chamber in the course of the operation, it is wise to remove

it, either by a gentle stroking movement of the *curette* over the cornea towards the incision, or, if that do not suffice, by carefully introducing the end of the *curette* within the incision itself. On two occasions only have I known the lens to become opaque after the operation, where the capsule was not directly injured (as it ought never to be) by the instrument. One of these was in my own practice, another in that of a highly esteemed colleague.

I am unwilling to extend this already long comment on the review by any inquiry as to the *rationale* of the operation; but there is much in favour of Von Graefe's original suggestion, that the diminution of intra-ocular pressure may be largely due to the lessening of the iris as the surface secreting the aqueous humour. My own opinion, however, was, and is, that the more direct communication opened between the vitreous and aqueous regions of the eye facilitates the play of currents between them, and thus allows an excess of fluid behind to come forward to the corneal surface, through which exosmosis is much easier than through the posterior coats, the sclerotic, choroid, and retina. This would go far to explain the apparently contradictory influence of the operation in raising the tension to the natural degree when previously diminished; for this also it is capable of doing in some cases, as shown long ago by Von Graefe himself. That the size of the piece excised in glaucoma has a direct relation to the effect produced is true according to his latest observations, confirmed I believe, by those of Donders, and of Arlt, the distinguished and most able Professor of Vienna. I can also add my own corroborative testimony. Von Graefe is thus led (in his latest memoir, just published) to insist once more, as he had done in his first paper, on the necessity of removing a considerable portion of the iris (not, however, so much as a third, or even a fourth, of its circuit), where it is wished to reduce permanently the intra-ocular pressure. A small iridectomy is insufficient, much less any mere puncture, such as some are still inclined to rely upon.

In conclusion, I may remind the reviewer, that after the reading of Mr. Hulke's excellent paper on the Pathology of Glaucoma, at the Medical and Chirurgical Society, on January 12, 1858, both Mr. Critchett and myself endeavoured, from our own experience, to enforce the importance of Von Graefe's new proposal (see the 'Medical Times and Gazette,' January 23), and that in our weekly operations at Moorfields, all comers have had the opportunity ever since of watching the progress of our numerous cases, as well as of those of our colleagues. Mr. Hulke advocated and explained this treatment in detail in an able paper "On the Surgical Treatment of Glaucoma," in the 'Medical Times and Gazette,' March 27, 1858, (see 'Retrospect,' vol. xxxvii, p. 265), referring to the favourable experience of Moorfields and to my own private practice, in which at that time I had the advantage of his assistance. He has recently reverted to the subject in a second paper at the Medical and Chirurgical

Society. To both these latter communications I venture to draw the attention of practitioners. The reviewer is in error when he states that there has been any holding back of opinions or of facts on the part of those who have introduced the operation into England.—*Medical Times and Gazette, Aug.* 25, 1860, *p.* 177.

85.—ON GLAUCOMA AND ITS SURGICAL TREATMENT.

By J. W. Hulke, Esq., F.R.C.S., Assistant Surgeon to King's College Hospital, and to the Royal London Ophthalmic Hospital.

[Our readers will do well to refer to previous articles on this subject, by Mr. Hulke, in vol. xxxvii, p. 265, and vol. xl, p. 222.]

Premonitory symptoms.—Rapidly increasing presbyopia; the appearance of a coloured halo round the flame of a candle; the spontaneous appearance of flashes and other spectra. Intercurrent obscuration of vision, attended with vague orbital and frontal pains, slight hardness of the eyeball, and contraction of the field of vision. The pupil is large and sluggish; the size of the anterior chamber is much diminished. The duration and intensity of these symptoms are very variable, but they are rarely absent.

Acute glaucoma.—The active stage sets in as a sudden and violent outbreak, often at night. Violent racking pain in the eyeball, often attended with sickness, and followed by rapid extinction of sight. The pupil is widely dilated and motionless; and the lens has sometimes the peculiar greenish tint which was formerly considered so characteristic. The ciliary vessels are swollen; the conjunctiva is red and often chemosed; the globe is very hard; the cornea is dull, and its sensibility is lowered. Remissions are followed by fresh paroxysms, and complete, irremediable blindness always ensues.

Chronic glaucoma.—The premonitory period slowly glides into the active. The obscurations, which were at first evanescent and separated by long intervals, become more frequent, and last longer. The contraction of the visual field progresses. The tension of the globe increases. The iris becomes dull; the aqueous humour turbid; the cornea dimmed and flattened. Mr. Hulke lays stress on the flattening of the cornea, which is easily demonstrated, because it has been recently stated that the cornea becomes conical in glaucoma.

Ophthalmoscopic signs.—Excavation of the optic nerve entrance, and pulsation of the retinal vessels. To these capillary apoplexy of the retina is often added; and sometimes there are small blood-clots in the vitreous humour, which is unnaturally firm. It is only late in the disease, when all the component structures are undergoing atrophy, that the vitreous humour becomes fluid.

The nature and causes of the glaucomatous process.—All the leading features of the glaucoma are due to excessive tension of the eyeball from a superabundance of fluid within it, which distends the

vitreous humour. This fluid—serum—is derived mainly from the choroid. Many circumstances show that the retina is only passively concerned. Glaucoma might be considered a serous choroiditis. Mr. Hancock has advanced the theory that spasm of the ciliary muscle forms an essential part of glaucoma. The author has, however, found complete atrophy of this muscle in some dissections of glaucomatous eyes; hence the inference that this muscle is not concerned in maintaining the glaucomatous condition. The author has been unable to trace any connexion between glaucoma and gout or rheumatism. Some other diseases and injuries of the eyeball occasionally assume a glaucomatous type. This is especially the case with wounds of the ciliary region and sclerotico-choroidal staphyloma.

Treatment.—Generally the age and broken health of the subjects of glaucoma forbid antiphlogistics; venesection is inadmissible; leeches and counter-irritants are useful as adjuncts, but cannot alone cope with the disease. The excessive tension of the globe is suggestive of the evacuation of some of the superabundant fluid by tapping. The old surgeons, Antonius, Nuck, Jobus, & Meekren, and others, were familiar with this operation, but they practised it chiefly in hypopion, onyx, and hydrophthalmos. Wardrop ('Med.-Chir. Trans.,' 1813,) tried it extensively. With a view to lessen fulness and congestion, he tapped the anterior chamber in superficial and deep-seated inflammations of the eye. The operation was at first warmly taken up by other surgeons, but soon fell into disuse. In our own day it has been strongly advocated by Desmarres, but it has found little favour with English surgeons, though most have occasionally performed it. In glaucoma the relief that paracentesis cornea affords is too transient to render it of much value. Paracentesis sclerotica has been practised by Desmarres and Hancock in glaucoma, though with different objects. Mr. Hulke reverts to this, after fully describing the operation of iridectomy as proposed by Dr. A. von Graefe.

Iridectomy consists in excising a segment of the iris, in its whole breadth, from the pupillary margin outwards to its insertion. This is effected through an opening of corresponding size at the extreme edge of the anterior chamber. Iridectomy may be practised at any part of the iris. Graefe usually makes it outwards; but adds that, when desirable for the sake of appearance, it may be made upwards. This latter position has been adopted by Mr. Bowman, and is that which Mr. Hulke has generally chosen. By removing the iris in this manner, the pupil is at once enlarged up to the corneal incision, which forms, as it were, the base of a coloboma iridis, and the edge of the lens, with the suspensory ligament, stretching in front of the vitreous humour and the ciliary processes, are exposed to view. The little blood which oozes into the anterior chamber from the cut edges or surface of the iris, should be at once pressed out or removed with a scoop. The after-treatment is very simple. A light compress may be applied for a short time as a precaution against hemorrhage. This

may be replaced after an hour or two by a piece of wet rag. The room should be shaded. Usually nothing else is necessary. At first the aqueous humour trickles away; but the corneal wound soon heals, and the anterior chamber fills again. The hardness of the eyeball is at once lessened, and a natural tension is gradually attained; the pain abates, and soon altogether disappears. As regards vision, the ultimate results are intimately dependent on the period at which the iridectomy is performed, being more perfect where it has been early undertaken than where it has been postponed. In the premonitory period, where the symptoms are well marked, the propriety of operating cannot be doubted. In acute glaucoma, where the operation is done during the first inflammatory attack, or soon afterwards, vision is very completely restored. In chronic glaucoma, the results are less uniform and less decided. This is in consequence of the insidious nature of the disease—structural changes in the retina creeping on *pari passu* with the gradually increasing tension.

Alleged objections to iridectomy.—1. Its *reported* uniform failure in the hands of some surgeons. This is in great probability to be generally attributed to its having been practised in cases which were not true instances of this disease. Many failures have proceeded from its having been done at far too late a period.

2. The great difficulty of the operation. This has been much magnified. It does not require more skill than most surgeons possess, and when chloroform is used it becomes really a simple matter; but even were it difficult, which it is not, in the absence of other known means of cure, we should be no more justified in rejecting it on this account, than we should be in refusing a patient the benefit of herniotomy where the taxis and other measures had failed.

3. The disfigurement produced by the coloboma iridis is so slight that it cannot constitute a real objection.

4. Its supposed injurious action on accommodation. Further experience has corrected some mis-impression which at first prevailed respecting its influence on the adjustment of the eye. The previously existing presbyopia is not increased by removal of a portion of the iris; indeed, the refracting power of the globe sometimes actually increases after iridectomy—probably, as Graefe has shown, in consequence of the flattened cornea resuming its natural curvative.

To avoid these alleged disadvantages, paracentesis scleroticæ has been advocated by Middlemore, Desmarres, and Hancock, as a substitute for iridectomy. Middlemore proposed to evacuate the *turbid, diffluent, vitreous humour* with a grooved needle, and to replace it with a syringeful of clear water. But, except in very old cases, the vitreous humour is much too firm to flow out along a grooved needle; and probably few English surgeons would adopt Desmarres' suggestion, of introducing a probe and breaking it up. Mr. Hancock, considering a spasm of the ciliary muscle to be an essential part of glaucoma, divides this muscle by striking a knife through the ciliary region back-

wards and inwards towards the axis of the globe. But Mr. Hulke has demonstrated, by microscopical examination, advanced atrophy of this muscle in many glaucomatous eyeballs; whence it follows that the ciliary muscle is not actively concerned in maintaining the glaucomatous process. In all probability, the success of Mr. Hancock's operation is solely due to the draining away of some of the superabundant fluid. According to this view, it is simply a peculiar mode of paracentesis, and cannot rank as a substitute for iridectomy until it has been thoroughly established that it *permanently* relieves excessive intra-ocular tension, which, in common with most surgeons, Mr. Hulke has found that tapping the vitreous humour fails to do.—*Lancet*, *July* 21, 1860, *p.* 61.

86.—ON THE DIVISION OF THE CILIARY MUSCLE IN THE TREATMENT OF GLAUCOMA AS COMPARED WITH IRIDECTOMY.

By Henry Hancock. Esq., F.R.C.S., Senior Surgeon to the Royal Westminster Ophthalmic Hospital.

[A great difference of opinion exists between Mr. Hancock, Mr. Hulke, and others, on the new operations on the eye for the relief of glaucoma. We cannot pretend to decide between these eminent ophthalmic surgeons, and therefore we prefer to let each party speak for itself. Mr. Hulke, in some very talented papers, has called in question Mr. Hancock's practice; and here Mr. Hancock defends himself, and, we confess, makes out a very good case. Mr. Hulke's operation is like that of Von Graefe's, namely, iridectomy. Mr. Hancock says that his operation of simply dividing the ciliary muscle is sufficient. He maintains that the appearances of the blood-vessels in glaucoma are due chiefly to the constriction exercised by the ciliary muscle, and that to remove this constriction it is only necessary to divide the muscle, and that this is the best operation. He says,]

My first operation for the division of the ciliary muscle was performed on the 9th of September, 1859; and on the 11th of February, 1860, I published an account of the operation, with cases; at the same time stating that I was led to adopt this mode of treatment from the belief "that the pathological and ophthalmoscopic appearances of the blood-vessels in glaucoma were due mainly to the constriction exercised by the ciliary muscle;" and that to remove this constriction I operated as follows:—"I introduce a Beer's cataract knife at the outer and lower margin of the cornea, where it joins the sclerotica. The point of the knife is pushed obliquely backwards and downwards until the fibres of the sclerotica are divided obliquely for rather more than one-eighth of an inch; by this incision the ciliary muscle is divided." This is the description of my operation which I published: it does not contain one word about "striking a knife through the ciliary

region towards the axis of the globe," which is the incorrect version given by Mr. Hulke—a proceeding which would be carefully avoided by any one conversant with the anatomy of the eyeball, from the risk of wounding the lens which would inevitably attend it.

As my operation has now been before the profession above twelve months, and still continues to be attended with the best results in the hands of my colleagues and myself, I cannot admit that iridectomy *is* the only known remedy for glaucoma. I propose, therefore, to consider, in the first place, the validity of Mr. Hulke's objections to my operation, as set forth in the paper already alluded to, and read before the Royal Medical and Chirurgical Society. Next, to inquire into the results of the operation of iridectomy, as furnished by Dr. Bader's papers in the 'Ophthalmic Hospital Reports' (Nos. 9 and 10); and lastly, to give the results of my own operation up to the present time, with my reasons for submitting it to the profession, not as the "only known," but as the *best* operation for the treatment of glaucoma.

1. The principal points upon which Mr. Hulke insists are, "That the leading features of glaucoma are due to excessive tension of the eyeball, from a superabundance of fluid within it, which distends the vitreous humour; that this fluid (serum) is derived mainly from the choroid; that it might be considered a serous choroiditis."

2. "That the excessive tension of the globe is suggestive of the evacuation of some of the superabundant fluid by tapping."

3. That he has "demonstrated, by microscopical examination, advanced atrophy of this muscle (ciliary) in many glaucomatous eyeballs; whence it follows that the ciliary muscle is not actively concerned in maintaining the glaucomatous process."

4. "To avoid certain alleged disadvantages, paracentesis sclerotic æ has been advocated by Middlemore, Desmarres, and Hancock."

5. "That in all probability the success of Mr. Hancock's operation is solely due to the drawing away of some of the superabundant fluid. According to this view, it is simply a peculiar mode of paracentesis, and cannot rank as a substitute for 'iridectomy,' until it has been thoroughly established that it permanently relieves excessive intraocular pressure," which, in common with most surgeons, Mr. Hulke has found that tapping the vitreous humour fails to do.

The assertion "that the leading features of glaucoma are due to excessive tension of the eyeball from a superabundance of fluid within it," and that "excessive tension of the globe is suggestive of the evacuation of some of the superabundant fluid by tapping," are contradicted and rendered untenable by other portions of Mr. Hulke's paper; whilst the results he gives of the operation of iridectomy directly prove, that the operation is only of value when the fluid, for the evacuation of which it is performed, is actually not in existence to be evacuated.

We are told that the reason why iridectomy has failed in the hands

of some surgeons "has proceeded, in many instances, from its having been performed at far too late a period.....that the propriety of operating in the premonitory period cannot be doubted.....that in acute glaucoma, where the operation is done during the first inflammatory attack, or soon afterwards, vision is very completely restored; whilst, in chronic glaucoma, the results are less uniform and less decided."

We may hence infer, that three stages of this disease are recognised: the premonitory, the acute, and the chronic, and that the success of the operation is greater the earlier it is performed. It is this great practical fact which seems to me to be fatal to Mr. Hulke's theory of glaucoma being due to a superabundance of serum distending the "vitreous humour," more especially as that gentleman, in the same paper, describes the vitreous humour as being "unnaturally firm in this disease;" and that it is only at a late period, when all the component structures are undergoing atrophy, that the vitreous humour becomes fluid, at which time the results of the operations are admitted to be "less uniform" and "less decided." And I am still further supported in the opinion I have expressed here and elsewhere, that fluid is not the cause, but the result, of the disease termed glaucoma, by the following very corroborative paragraph, extracted from Mr. Hulke's paper on the 'Pathology and Morbid Anatomy of Glaucoma,' read before the Medico-Chirurgical Society, Dec. 12th, 1857: —"With a view to relieve the tension of the globe, I have seen the sclerotic freely punctured with an extraction knife, after which firm counter-pressure with the finger upon the opposite side of the globe only caused the protrusion of a very small bead of yellowish vitreous humour, such great firmness had it." Whether the tension of the globe was relieved by the puncture is not stated.

Nor is it by any means so conclusive as Mr. Hulke appears to imagine, that, because he has "demonstrated advanced atrophy of the ciliary muscle in many glaucomatous eyeballs, this muscle is not concerned in maintaining the glaucomatous condition." The word "many" is very indefinite and inconclusive. Mr. Hulke does not state in *how* many instances he has found this muscle atrophied, or what proportion these instances bore to the number of glaucomatous eyeballs which he examined microscopically. He does not inform us whether these glaucomatous eyeballs were obtained after the death of the patient or before, or, if from the former, the time which had elapsed between the death and the examination; for the changes which take place, especially in diseased eyes, are so rapid, that very little reliance can be placed upon such examinations when they have been deferred for any length of time. Neither, where the glaucomatous eyes which he examined had been obtained from living patients, does he tell us the stage of the disease at which they were extirpated, or the circumstances which necessitated their extirpation. It had been but justice that these particulars should have been mentioned before a sweeping pathological statement was unhesitatingly advanced.

I have already alluded to the three stages of glaucoma described in Mr. Hulke's paper—the premonitory, the acute, and the chronic; to his admission that it is only late in the disease that the component structures of the eyeball undergo atrophy; and that whilst iridectomy is most successful during the first two stages, its results are less uniform and decided in the last; whilst Dr. Bader states, in his report, "that the prognosis of chronic glaucoma depends upon the stage in which the eye affected is operated upon: when blind for some time, it is not expected to regain sight; a chronic glaucomatous eye, with mere perception of light, is rarely improved by operation."

Mr. Hulke's microscopical examinations, therefore, cannot be admitted as at all conclusive or of much value against my proposition, "that the ciliary muscle exercises considerable influence in maintaining and aggravating the glaucomatous condition," and still less against my operation for the division of this muscle. They only prove that in certain cases of advanced glaucoma, in which an operation is admitted to be rarely successful, the ciliary muscle may have undergone atrophy with the rest of the tissues; but they by no means prove that this is the case in all or even in average instances, as we cannot for one moment imagine that any surgeon would extirpate a glaucomatous eye capable of relief by operation, unless under very peculiar circumstances. I presume it is only recently that Mr. Hulke has discovered this condition of advanced atrophy of the ciliary muscle in glaucomatous eyeballs, otherwise he would scarcely have omitted all mention of a fact of so much importance in his paper upon the "Pathology and Morbid Anatomy of Glaucoma," read before the Royal Medico-Chirurgical Society in December 1857.

On the other hand, my friend and colleague, Mr. Hogg, has kindly furnished me with the particulars of two glaucomatous eyeballs, extirpated at an advanced period of the disease, in which his microscopical examination demonstrated the ciliary muscle as highly developed, and anything but in a state of atrophy. I do not, however, advance these cases as of themselves sufficient to controvert Mr. Hulke's assertion —their number is too small; but, at the same time, they afford pretty conclusive proof that the ciliary muscle is not atrophied in all cases even of advanced chronic glaucoma. Neither can I admit that the success of my operation in any way depends upon, much less is solely due to, the drawing away of some of the superabundant fluid; or that it is a peculiar mode of paracentesis, to be classed with the operations of "paracentesis scleroticæ" of Middlemore and Desmarres.

I think I have succeeded in showing that, according to Mr. Hulke's own statements, there is no superabundance of fluid in those cases most likely to be benefitted by the operation; therefore the drawing away theory falls to the ground, and is still further negatived by cases Nos. 15 and 16, appended to this paper.

The object for applying the term paracentesis scleroticæ to my operation, and classing it with the procedures of Middlemore and

Desmarres, is transparent enough. Paracentesis of the cornea and sclerotica, as practised by these two surgeons, has not met with any great amount of success. If, therefore, the profession could be impressed with the notion that my operation was nothing more than one or the other of these proceedings, and that any transient good which it might be the means of effecting was due to the mere evacuation and draining away of fluid, it would be looked upon as deficient both in value and originality, and would, as a matter of course, fall into disrepute, and proportionately give greater prominence to the operation—iridectomy. It is true that the word paracentesis means literally, a "piercing through," but its application in surgery has hitherto been restricted to the operation of tapping. If we were to attempt to describe the operation of tenotomy in club-foot as "a peculiar mode of paracentesis" of the leg, or of the foot, or if we were to designate the operation for the extraction of hard cataract as "a peculiar mode of paracentesis" of the cornea, we should expose ourselves to the charge of pedantry; yet the name may with equal propriety, be applied to these operations, or even to iridectomy itself, as to my operation for the division of the ciliary muscle. Hence the term as applied to this operation by Mr. Hulke is a misnomer.

The operations of Middlemore and Desmarres were introduced for the avowed purpose of relieving intra-ocular tension by the evacuation of fluid. My operation, on the contrary, is introduced for the avowed purpose of relieving the constriction of the several coats of the eye by division of the ciliary muscle.

An increased quantity of fluid may or may not be present, and, when present, some may flow by the side of the knife; but this is merely a coincdence, not by any means the primary object of the operation; for mere evacuation of fluid without division of this muscle is quite incapable of affording permanent benefit.

In my former paper I pointed out the variety of opinions entertained by the supporters of iridectomy in this country as to the *modus operandi* of the operation. I would here venture to suggest another. I believe it will be found in the course of time, that the element of success is the same in iridectomy as in my operation—viz., "the division of the ciliary muscle." That, from the situation in which Von Graefe makes his first incision, he at the same time cuts it through, and I have very little doubt it will ultimately be found that the extent of this incision may be advantageously curtailed, and the tearing away of the iris altogether dispensed with.

[Mr. Hancock then goes on to say in another part of his paper:]

I have now to offer a few observations upon the results of my operation up to the present time.

I also propose to add the present condition of several patients upon whom my operation was performed some months since; and I do this with the greater confidence since Mr. Hildige, in commenting upon

the operation when first announced, fell into the error of assuming it was mere paracentesis, which he asserted produced merely temporary benefit, the symptoms returning in from one to three months; whilst it has been expected that the profession would accept and become devotees to the operation of iridectomy from a number of operations, the results of which are calculated at the comparatively short interval of from two to three weeks from the time of operation.

I have always felt that Mr. Hildige's objection possessed great weight, and my attention has consequently been carefully directed to this point, and the result has been to strengthen me very considerably in my original conviction, that my operation did not relieve by evacuating fluid, but by removing constriction. I scarcely know of a single instance in which the patient was benefited immediately, or even shortly after the operation, experiencing the disappointment of a relapse; whilst, on the contrary, in very many instances, the relief has been so gradual that, for some three or four weeks, it has been scarcely appreciable.

The class of cases in which improvement is most rapidly manifested, is that of acute and subacute glaucoma, and what has been termed "evanescent obscurations." In chronic cases, and those of total blindness, or where a mere perception of light has remained to the patient, the improvement has been of much more tardy manifestation, rarely taking place under a fortnight from the operation, after which it usually increases, and rarely, if ever, recedes.

The great value of this operation seems to me that by an almost painless proceeding, however severe the previous suffering has been, the pain is almost invariably removed. Not only is this the case, but, if fairly tried (which I am justified in asserting it has not been by those who appear to be so anxious to disparage it), it will be found that whilst capable of effecting an equal amount of good as "iridectomy," it is free from the danger and objections attending this operation. For instance,—

a. It is much more easily and rapidly performed, and the incision made into the eyeball is much smaller.

b. The operation is attended with so little pain that there is no necessity for chloroform; the sickness and accidents resulting therefrom are consequently avoided.

c. As the eye does not require to be fixed by forceps, the danger of lacerating the "rotten" conjunctiva, or of rupturing the suspensory ligament of the lens, is likewise avoided.

d. The incision is made at once by a Beer's knife, and does not in any case require to be enlarged by scissors.

e. As the incision commences at the margin of the cornea, and is carried obliquely into the sclerotica away from the cornea (not backwards and inwards towards the axis of the globe, as erroneously stated by Mr. Hulke), and as the iris is left intact, and is not to be seized by forceps nor drawn out by a hook, there is no danger of the

lens escaping, being broken up, or so injured as to cause traumatic cataract.

f. The integrity of the iris being preserved, the operation is not necessarily or even beneficially restricted to the upper segment of the cornea in order to obtain the compensatory support of the upper lid. The patient, moreover, retains the power of adapting his vision to near objects; and in the majority of cases there is no permanent disfigurement of the eye, or coloboma iridis.

g. The iris not being injured, hemorrhage from its lacerated edges cannot occur; thus it can neither be necessary to employ a scoop to remove the blood, nor to make a counter-opening to admit of its escape.

h. The incision is so small that it is impossible for the lens or the vitreous humour in any quantity to escape through it. The necessity of extirpating the eyeball, therefore, from either of these causes, or on account of hemorrhage, as I have already shown, cannot by any possibility occur.

i. It relieves pain by the removal of the constriction of the eyeball, and the consequent pressure upon the nerves from the undue constriction of the ciliary muscle.

j. By it the impediment to circulation through the blood-vessels *is* got rid of; they are placed in a favourable condition to recover their normal state, and the probability of a recurrence of the effusion is greatly diminished.

k. Where it fails, it does so little damage to the eye, that iridectomy may subsequently be performed, if considered beneficial to the patient.—*Lancet, Oct.* 13, 1860, *p.* 355.

87.—*On Division of the Ciliary Muscle, in Acute Glaucoma.*—Of the value of Mr. Hancock's important and successful operation of division of the ciliary muscle, we (Ed. of Lancet), have now abundant evidence; and to-day we record an illustration (selected from amongst a number in the Westminster Ophthalmic Hospital) which we had the opportunity of personally examining, and of witnessing the good effects produced, in a female nearly sixty years of age, affected with acute glaucoma. The patient was blind on admission, but in three weeks she was able to read large-typed words in the Bible. It was a case well verifying Mr. Hancock's views.

A surgeon, recently writing from Paris, assured Mr. Hancock that he had the satisfaction of receiving M. Desmarres' testimony regarding the results of this operation in a case of glaucoma in which he had tried it. It was described as a very bad case, and the effects of the operation were all that could be desired, quite as complete as after the performance of iridectomy. It was the intention of M. Desmarres to continue the practice, more especially in his bad cases, one-half of which he should treat by paracentesis, the other half by Mr. Han-

cock's method. In our "Parisian Medical Intelligence" for May 19th, the successful result of the latter procedure in a case of acute glaucoma of extreme severity, in the hands of M. Desmarres, is thus announced: "Within twelve hours of the operation all the symptoms had improved, and at the present moment (it is now five weeks since the event) the patient is fully as much benifited as if the most successful iridectomy had been performed." It will be seen, therefore, from the results already obtained in London and elsewhere, that the success of the operation is established, and Mr. Hancock has thus rendered a most important service to ophthalmic science.

For the notes of the following case we are indebted to Dr. Sansom, the medical registrar to the hospital:—

Harriet B——, aged fifty-eight, was admitted into the hospital on the 21st of March. She complained of intense pain in the right globe, and throbbing pain over the brow: the sight was wholly gone; the conjunctiva was greatly congested; the sclerotic presented a purplish glow; the pupil was dilated and irregular, and the lens opaque and disorganised, presenting a greenish appearance. The light used for opthalmoscopic examination could not penetrate the disorganized lens. The eyeball was exceedingly tense, but the slightest pressure caused most severe pain. The patient was thin and pale, and presented the appearance of having undergone much suffering. Her previous history was as follows:—She had never had any disease of the eye until the present attack. Three months ago the eye watered as if she had caught cold in it, but she paid little attention to this. Occasionally, towards evening, she experienced the sensation of flashes of fire before the eye, and the flame of the candle seemed surrounded by a halo of colour, but all unpleasant symptoms disappeared towards morning. On the 14th of March (a week before admission) it rapidly became worse, great pain occurred over the brow, and a tense throbbing pain referred to the back of the orbit. The eye, she said, seemed so tight that it would burst. The symptoms continued to increase.

On March 23rd, Mr. Hancock divided the ciliary ligament, and a quantity of fluid and disorganized vitreous humour escaped. The eye was kept from the light.

March 26th. The pain has entirely left her; she is able to tell the number of fingers held up before the eye which has been operated on, the other being closed; the iris has been slightly drawn into the wound.

30th. Pain continues entirely absent. She can now distinguish the outline of any object, and she says that the sight is improving every day.—*Lancet, July* 7, 1860, *p.* 7.

88.—CASES OF GLAUCOMA TREATED BY DIVISION OF THE CILIARY MUSCLE.

By JAMES ROUSE, Esq., Surgeon to the Royal Westminster Ophthalmic Hospital.

As it is possible that some of our readers may not know the exact steps of Mr. Hancock's operation for the relief of glaucoma, the following extract from that gentleman's lecture, published in the 'Lancet' of Feb. 11th, may be deemed acceptable:—"I introduce a Beer's cataract knife at the outer and lower margin of the cornea, where it joins the sclerotica; the point of the knife is pushed obliquely backwards and downwards, until the fibres of the sclerotica are divided obliquely for rather more than one-eighth of an inch: by this incision, the ciliary muscle is divided, whilst the accumulated fluid flows by the side of the knife.

If the reader will refer to the lecture from which the above extract is taken (see Retrospect, Vol. xli, p. 253), he will find that Mr. Hancock was induced to perform the operation just described from having noticed in several cases of acute glaucoma a peculiar constriction around the cornea corresponding to the situation occupied by the ciliary muscle; the operation was followed by most satisfactory results.

The following cases are the more interesting, inasmuch as they did not present that peculiarity; and it will be seen that the operation was equally successful.

Case 1.—H. W., aged 46, married, the mother of children, was admitted under my care on May 4th. During the last eleven years, she had suffered from occasional attacks of pain (accompanied by dimness of vision) in the left eye. Two months ago she was attacked with pain in the left eye, which prevented her from following her usual occupations. A week ago the pain became so intense that she could not procure sleep. During this time her sight was very limited; she could not see more than two yards before her, and then the objects looked confused; beyond that distance, everything appeared to be dense fog. She complained of intense throbbing pain in the eyeball of the left eye, circumorbital pain, and occasional flashes of light. The pain was more severe at night, and at times appeared to affect the whole head. She could not distinguish any object. On examining the eye, the cornea was found to be slightly ulcerated. The anterior chamber was very much flattened, partly from pushing forwards of the iris, and partly from the stretching of the cornea from the intraocular effusion. The pupil was large and immovable. The conjunctival and choroidal veins were large. Pressure on the eyeball caused extreme pain. She had had no sleep for a week.

May 4th, 2 p.m. The ciliary muscle was divided; a considerable amount of fluid escaped. Within an hour after the operation the pain was relieved, and she passed a most excellent night.

May 18th. She had had no pain since the operation; and, on a watch being given to her, she could tell the time; and could distinguish silver from gold. The eye had nearly regained its natural appearance. The anterior chamber was of the same depth as that of the right eye. The ulceration of the cornea had healed, leaving only a slight cloud.

May 28th. She had had no return of pain, and saw perfectly. The cornea was rapidly clearing. She was made an out-patient.

June 15th. She came to show herself, stating that she was perfectly well.

Case 2.—A. H., aged 47, married, the mother of seven children, looking at least ten years more than her age, was admitted on May 18th under my care. Four years ago, her sight began to get weak, not being able to thread a needle without glasses. One night in the middle of last September, she was attacked with violent pain in both eyes and around the orbits, especially the left. The following day, the pain increased in intensity, and compelled her to keep her bed. The sight of the left eye became seriously affected, vision being very limited and indistinct. The right eye was affected, but in a less degree.

In December, she came to the hospital as an out-patient; at that time the sight of the left eye was completely lost; the pupil was large and somewhat irregular; the vitreous body was too opaque to allow ophthalmoscopic examination. The pain was so intense in that eye and side of the head, that she could not sleep. Under these circumstances, the ciliary muscle was divided. The operation completely relieved the pain; but there was no return of vision. The right eye improved very much after the operation; in fact, she was so much better that she ceased to attend.

May 16th. She again applied for advice; the right eye had become so much worse, that she could no longer see to read. She could distinguish large objects, but not sufficiently to recognize a face. The pupil was large and fixed, the cornea was flattened, and the iris pushed against it. The vitreous body was semi-opaque. She complained of great pain in the right temple and side of the head. She felt so giddy, that she did not like to move about.

May 18th. She was admitted into the hospital; and the ciliary muscle was divided.

May 30th. She had progressed favourably since the operation, the mist had entirely gone, and she was able to read for a short time without inconvenience, but it caused subsequent uneasiness. She stated that she could not look very long at an object without the eye becoming slightly "dazzled." She was made an out-patient.

June 15th. The sight was improving—every day becoming stronger. She could now look at an object for a much longer time without inconvenience. She saw everything perfectly.

August 13th. The sight perfectly good in the right eye. She finds

that if she takes cold there is slight mistiness for a day or two. During the last month there is slight return of sight in the left eye. She can now perceive the window-frames.

Case 3.—L. N., aged 30, single, a dressmaker, was admitted under my care on June 8th. Two years since the sight began to get dim and misty; she had had pain from time to time in the temples across the forehead; after each attack the sight was less good. A year ago, she became decidedly worse. The pain in the eyes and temples was more severe, and the sight was so much affected that she could no longer see anything distinctly. She could only read large print, and see to do the coarsest work, and this very indifferently. She had not menstruated for two years.

On admission, the pupils were large and fixed; the refractive media were turbid. Ophthalmoscopic examination showed dilated veins on both retinæ. The optic discs could only be seen imperfectly; they appeared slightly cupped. The eyes were very hard, and the anterior chambers and cornea were flattened. She complained of iridescent vision, and pain in the eyes and head. She was given a watch, but she could not positively tell the face from the back. She thought she could distinguish very large type, but the attempt to read caused pain and dizziness in the head. The ciliary muscle was divided in both eyes.

June 18th. She had had no pain since the operation. The irides had assumed the natural size, and acted perfectly. This morning she threaded a needle, and read some ordinary sized print. She said she had not seen so well since the first attack two years ago.

August 13th. The sight continues satisfactory.—*British Medical Journal, Aug.* 18, 1860, *p.* 645.

89.—ON GLAUCOMA AND THE OPTIC PAPILLA.

By Dr. Mackenzie, Glasgow.

I have long since established to my own satisfaction the three following conclusions regarding glaucoma;—

1st. That the sea-green appearance behind the pupil, whence the name arose, does not depend, as was once generally supposed, on any thickened or discoloured condition of the vitreous humour, but on a diplochromatic state of the crystalline, by which it absorbs the extreme prismatic rays, and reflects the middle ones.

2nd. That the vitreous fluid in glaucoma is superseded by an unhealthy secretion, which over-distends the eye, makes it feel hard, causes severe pain by pressing on the ciliary nerves, and obliterates the sensibility of the retina; a state of matters which I found to be susceptible of relief by paracentesis of the eye, through either the sclerotica or the cornea.

3rd. That in advanced cases, the optic nerve behind the eye shows

itself, on dissection, in a state of atrophy, deprived more or less of its proper nervous substance, and flattened.

Ophthalmoscopic investigations have added some new facts to our knowledge of glaucoma. One of these is the pulsation of the arteries of the retina; and another, the concave or excavated state of the papilla of the optic nerve. Neither of these phenomena, however, is peculiar to glaucoma; both of them being occasionally met with in other diseases of the eye. The former of them seems to indicate an increased resistance to the flow of blood in the vessels which supply the retina; the latter may be regarded as the effect of one or other, or of both, of two different causes, namely, the atrophied condition of the optic nerve, and the increased pressure on the internal parietes of the eyeball, from the superabundant secretion which occupies the place of the vitreous humour.

These phenomena can scarcely be discerned in the advanced stages of glaucoma, owing to the diplochromatic and muddy state of the crystalline. To see them, then, the student should be directed to examine a case, in which the dioptric media have as yet lost but little of their normal colour and transparency.

To discern the pulsatory movement of the vessels, requires a sharp and experienced eye, and the observer will find it of service, while directing his attention to this point, to have the patient's head supported, and the diseased eye steadied by the fingers of an assistant.

If the indirect or inverted method of ophthalmoscopic observation be selected, an optical decption is apt to bewilder a beginner, as to the condition of the entrance of the optic nerve, a nearly circular spot, though not unfrequently oval, variable in size, but measuring on an average 0·6 line in diameter, and which, although styled *papilla* or *colliculus*, is, in its normal state, nearly level with the retina, and even a little depressed in its centre. In the direct method, where the observer regards the illuminated non-inverted fundus oculi through an aperture in a concave mirror without the aid of any extraneous lens, the great magnifying power of the cornea and humours of the patient's eye, gives to the papilla an apparent magnitude larger even than that of the pupil; but in this way, it cannot be well seen as a whole, and generally requires the eye of the observer to be brought inconveniently near to that of the patient. Instead, then, of using the patient's eye as a powerful single microscope, in actual contact with the objects on the fundus oculi to be examined, and thus viewing them directly, it is better to obtain a smaller but more defined image, although an inverted and virtual one, of those objects, by converting the patient's eye for the time into the object-glass of a compound microscope, which we do by holding in front of it a thick convex lens. The image which we then see of the several parts of the fundus oculi is an inverted one, like that of an object examined with any ordinary compound microscope; the entrance of the optic nerve is seen towards the temple instead

of the nose; the macula lutea appears to the nasal side of the optic nerve instead of the temporal, and a little below the level of the nerve instead of above it; while the principal trunks of the retinal vessels, instead of branching in the direction of the temple to embrace the macula lutea, seem to bend towards the nasal side of the eye. The apparent position, in fact, of all the objects on the fundus, viewed in this way, is the reverse of their real position.

The most important optical deception which arises from viewing the fundus in the indirect method, as well as the most puzzling to a beginner, affects the papilla. The student has probably heard, that the papilla, in the glaucomatus eye, is cupped or excavated, but to his view it appears quite the reverse, it appears rounded and prominent.

To comprehend clearly that this is an illusion, all that one requires to do, is to impress with the head of a pin, a small dimple on a bit of paper, and put this under a compound microscope, with the concave side of the impression uppermost. The same appearance will then be seen, which is presented by the papilla of the glaucomatous eye, viz., that of a rounded and protuberant surface.

This optical deception arises from the inversion which the image suffers by being viewed through the compound mioroscope. We judge that an object, viewed with a single eye, is convex or concave, solely by the manner in which light is reflected from the body under examination. The light which falls obliquely on a convex surface illuminates that side which is nearer to the source of light; the side farther from it is in shade. The light which falls obliquely on a concave surface illuminates that side which is farther from the source of light; the side nearer it is in shade. Let the source of light remain in the same position, but invert the image of the object illuminated, so that the light which falls on the farther side of it may seem to fall on the near side, which is the case when we look through the compound microscope at the hollow on a bit of paper, or when we examine the optic papilla through the compound microscope formed for the occasion by the patient's eye *plus* the convex lens held in front of it; and both the dimple on the paper and the papilla, although they are actually cupped or concave, will then appear convex and prominent.

If, on the other hand, we turn the convex side of the dimple on the paper uppermost, and view it with the compound microscope, it appears concave. The inversion of its image causes the light which falls on its near side, to appear as if it fell on its farther side, and thus the eye is subjected to a deception the reverse of the former, and from which it cannot free itself. If there be cases, then, in which the end of the optic nerve within the eye actually projects in a convex form, they will offer, when examined in the indirect method, the appearance of a cup or depression.

Such facts have long been familiarly known; the apparent transmutation of an intaglio into a cameo, or that of a cameo into an

intaglio, under the compound microscope, being a common source of amusement, fully discussed by Sir David Brewster in his 'Letters on Natural Magic, Letter V.' Important as their bearing is on pathological examinations of the eye, they seem to have escaped the notice of ophthalmoscopists, till attention was directed to the subject by Dr. A. Weber, in a paper in the 'Archiv für Ophthalmologie,' Band II, Abtheilung I, Seite 141.

In a highly interesting communication in the same journal (Band IV, Abtheilung II, Seite 1), Dr. H. Müller has directed attention to the normal, as well as to several diseased states, of the optic papilla.

His account of the entrance of the optic nerve into the eye, and the diagram which he gives in illustration, tally remarkably with the thirteenth figure in Mr. Bowman's "Lectures," and the corresponding explanation. From the statnment and figures of these two observers, especially those of Dr. Müller, it may be gathered that the lamina cribrosa is normally somewhat concave towards the interior of the eye; that the fibres of the optic nerve, suddenly losing their white substance and dark outline, enter the eye on a level with the chorio-capillaris; that at this point the whole nerve, from the change which its fibres have just undergone, is considerably and rather suddenly reduced in thickness; that the fibres, bending more or less abruptly outwards, and spreading around, become clothed by the exterior or radially disposed layers of the retina; that the edge of the opening through which the nerve passes into the eye, as well as the fibres themselves as they traverse that edge, form a slight elevation or approach to a papilla, leaving in the situation where the trunks of the central vessels of the retina generally make their appearance, a small foveola.

Dr. Müller points out the difficulties which attend the anatomical examination of this depression, arising from the softness of the part, and its liability to change on being touched; and shows how these difficulties may best be obviated. He directs attention to the varieties which exist in different individuals; varieties in the depth of the depression, from 0·2 to 0·5 millimetre; varieties in its form and position, for it is not always symmetrical, and does not always correspond to the middle of the nerve, but is sometimes considerably nearer to the macula lutea, or has one portion of its edge more raised than the rest; varieties in the disposition of the vessels, for while the large trunks generally emerge from the middle of the foveola, one or more sometimes seek a passage for themselves close to the edge of the chorio-capillaris, so as to make their appearance by the side of the nerve; facts, all of which should be carefully borne in mind by opthalmoscopists.

In regard to abnormal prominence of the papilla, Dr. Müller shows, that this is likely to arise from an atrophied state of the exterior layers of the retina, a thickened condition of the primitive nervous

fibres, as well as infiltration of the nerve by blood, inflammatory exudation, or new formations. He relates a case in which a concretion in the site of the lamina cribrosa caused a protuberance of the papilla. The very earliest stage of encephaloid tumour should show an abnormal prominence of the papilla.

On the other hand, intra-ocular pressure, and atrophy of the nerve, existing either singly or in combination, are the causes of abnormal excavation of the papilla. In case of intra-ocular pressure, either simple, or combined with atrophy of the nerve, paracentesis, either corneal or sclerotical, is likely to produce a change in the depth of the excavation, sufficient to be recognized on ophthalmoscopical examination.

Morbid excavation of the papilla varies in depth, reaching in extreme cases to a millimetre beyond the level of the choroid. In such cases, the sides of the excavation are nearly perpendicular, or are even concave, and its edge, of course, impendent, so as partially, or or even completely, to hide from view the course of the vessels as they pass from the bottom of the excavation and over its edge to the retina. In slight cases, the sides of the excavation are convex, and its form that of a funnel. The lamina cribrosa, under such circumstances, keeps its place, but in more advanced stages, it is pushed back, and the excavation is much extended laterally, Such extreme cases of expansion are not the result of uncombined atrophy of the nerve, but must arise from the supporting resistance of the parts being weakened by previous inflammation, whence they come to yield more readily to intra-ocular pressure.—*Ophthalmic Hos. Reports, No.* 11, *p.* 252.

90.—ON THE OPHTHALMOSCOPE AND ITS APPLICATION.

By Haynes Walton, Esq., F.R.C.S., Surgeon to St. Mary's, and to the London Ophthalmic Hospitals.

[Mr. Walton believes that the origin of the ophthalmoscope is to be traced to a communication]

In the twenty-ninth volume of the 'Medico-Chirurgical Transactions,' by William Cumming, Esq., formerly house-surgeon to the London Hospital, "On a Luminous Appearance of the Human Eye and its application to the Detection of Disease of the Retina and Posterior Part of the Eye."

[It would appear that Mr. Cumming's attention was attracted to the subject from the description of cat's-eye amaurosis by several authors; i.e. amaurosis in which there is a luminous glare like that occasionally seen in the eyes of some animals, this Mr. Cumming shows to be natural to the healthy human eye, when the pupil is widely dilated and the eye seen in a certain light.]

His simple process of examination is this:—"Let the person under examination sit or stand eight or ten feet from a gas light, looking a little to the side; standing near the gas-light, we have only to approach as near as possible to the direct line between it and the eye to be viewed, at once to see the reflection. Or, in a dark room, a candle being placed four or five feet from the eye, if we approach the direct line between them, we shall be able at once to see it in many cases. If solar light be admitted through a newly closed shutter into a dark room, the luminosity may be seen when the pupil is tolerably dilated, the patient standing five or six feet from the aperture, and the observer occupying the position before indicated."

"These, then, are the circumstances necessary for seeing the luminosity. That the eye must be at some distance from the source of light, the distance being greater in proportion to the intensity; that the rays of light diffused around the patient (and sometimes around the eye itself) should be excluded; that the observer should occupy a position as near as possible to the direct line between the source of light and the eye examined: hence it is sometimes necessary for the observer to stand obliquely, that his eye may approach nearer to the direct line."

"The appearance of the reflection itself not only varies much in colour and intensity in different persons, but also from the circumstances under which it is seen; viz., the greater or less intensity of light, the position of the eye examined, and the distance at which it is viewed." Many other similar modes are given.

Three times I have seen the eye accidently illuminated. The first time that I was called to a consultation in the country (an occurrence which a man is not likely to forget), I saw a beautiful illumination of the eyes of a gentleman who was sitting in the corner of the waiting-room of the old station of the Great Western Railway, a few feet behind a gas-lamp; and associated with it was this coincidence, that the gentleman, himself blind, who came to town with his servant to take me to one of his pupils, who had been wounded in his eye, was at the time telling me of his own blindness, and called it "cat's-eye amaurosis." After that, I witnessed the same thing in a lad, behind the gas-light, in a druggist's shop. I again saw it in my study by my microscope lamp.

Mr. Cumming's paper, with all its experiments and suggestions, and not less its originality and philosophy, bases all the application of the discovery in the mere illumination of the interior of the eye-ball; full illumination being taken as the indication of health, and the absence of it as indication of disease in the retina and posterior part of the eye; for instance, he writes:—

"The establishment of the fact of a similar reflection from the healthy human eye to that from the eyes of animals, appears to me chiefly important in its adaptation as a mode of examining the posterior part of the eye. The retina and choroid, hitherto concealed in

the living eye, and little opportunity being afforded of examining their condition after life, in consequence of their diseases not terminating fatally, considerable uncertainty has attended the diseases ascribed to these structures; but the existence of this luminosity having been recognised, its non-existence or abnormal appearance may enable us to detect changes in these structures heretofore unknown, or satisfactorily to see those which we only suspected."

If we dilate the pupil by atropine, we have a means afforded of seeing the condition of the retina and choroid in every case. The cases I have examined in this way have confirmed the general impression that the retina is not frequently the seat of change in amaurosis; for, out of several cases of amaurosis, in which the non-opacity of the cornea, lens, and humours, allowed this mode of examination, I found but two, in which the retina was so changed that the reflection was not seen."

Among the physiological conditions of the eye enunciated by Mr. Cumming, are these: that the choroid is the principal reflecting structure, the light returned from the retina and concavity of the hyaloid body doubtlessly increasing the effect; that the brilliancy is considerably augmented from the concentrating influence of the concave shape of the retina, and the focal distance of the lens; that the luminosity of the healthy eye appears to be in proportion to the light colour of the pigment. The reflection from the albino is evident in ordinary day light. He further observes:—

"On placing a middle aged man, an albino ten or twelve feet from a gas-light, the reflection was extremely vivid, and of a pinker colour than ordinary, while the light transmitted through the choroid and iris evidently increased the effect. On placing close to his eye a black card with an aperture a little larger than the pupil, the reflection was brighter than that from the eye of a fair person examined side by side, but was of a more decided pink colour.

"In persons of fair complexion and blue or grey irides, it is generally more brilliant and more readily seen than in those of dark skin and irides. In the mulatto, it is also dusky; but in them, as in persons of swarthy complexion, a silvery reflection is sometimes seen, and is most probably a reflection from the retina. In the albino, this reflection produced by the vascular choroid is most brilliant and lightest in tint. In proportion to the darkness of the pigment, its lustre is diminished and the colour becomes more dusky."

Perhaps there appears a contradiction here, but there is really no discrepancy. The choroid is found to reflect greatest where the pigment is wanting. The pigment has nothing to do with the reflection; on the contrary, it interferes with it, and therefore lessens the illuminating power. In man, the pigment is less abundant and less intense in colour than in many of the lower animals; it is brown, rather than black.

It was then the first grand principle, the comprehensive idea, that

Mr. Cumming gave us. He did not see the optic disc, nor the retinal vessels. The perfecting of the discovery and the practical application, involving as they did the difficulty of inventing an instrument and the working out of much detail, were reserved for other labourers. These were found in Germany. It would be fruitless to trace the instrument through its stages of development. I will merely give the names of its several improvers, as far as I can ascertain, in the order of time: they are, Helmholtz, Ruete, Coccius, and Anagnostakis.

The ophthalmoscope at present in use is a portable little concave mirror, made of silvered glass, having a hole of sufficient dimensions in the centre, with a handle of about ten inches. The hole is usually too small; it should be of sufficient size to admit of distinct vision when the instrument is held with the requisite obliquity; the edges should be smooth and free from splinters. A double convex lens, of from two and a half to three inches focus, is needed as an appendage; but the power should be regulated by the usual range of the observer. It should be set in a frame with a stem of three or four inches long, which renders it more readily used. I know that mirrors of different curve and lenses of varying power compose the apparatus of some surgeons. That these varieties can be brought into requisition according to one's fancy, is undoubted; but I believe that they possess no utility, no decided practical advantage.

The ophthalmoscope demands an artificial light. Under an emergency, I have employed a candle, and with the faint illumination have gathered some, although an imperfect, idea of the interior of the eye. A steady lamp-flame is needed for the full effect. Whenever I can command it, I employ gas, and use a small argand burner, at the bottom of which is an arrangement with glass collar that ensures a steady flame, that slides on a vertical rod with a pedestal. It is a gas-lamp much used by the artizans of the watchmaking district of Clerkenwell, and sold at a lamp shop in Clerkenwell Green. A very light blue chimney whitens the light by reducing the predominant red rays, and the eye is then more naturally seen.

The examination should be made in a dark room.

As a rule, the eye should be prepared by having the pupil dilated by belladonna or some of its salts. Now and then persons are met with, especially fair young persons, whose naturally expanded pupils enable us to dispense with the artificial enlargement, particularly if it be not more than a mere glance within the eye that is needed; and above all, near-sighted eyes may be so examined; but generally, in adult age, and still more in old age with its discoloured lens, and in all cases where a thorough survey is called for, the natural pupil will not suffice. Vain discussions have been wasted on this question, which may be resolved into this common-sense remark, that if the desired object can be accomplished without interfering with the pupil, all the better, as the sequel will show; if it cannot, dilatation must be resorted to.

The pupil is generally more contracted by the light of day, than by the light of the ophthalmoscope. Often when it has appeared too small to allow of any serviceable examination, it has dilated sufficiently when the room has been darkened, and remained of such capacity as to afford all that was desired.

Unnecessary dilatation should always be avoided, because this unnatural state produces positive discomfort from indistinct vision, the degree and duration of which, from days to weeks, is proportionate to the dilatation and the healthiness of the eye. Patients frequently leave the surgeon on account of this, even when they have been forewarned. In spite of all assurance to the contrary, they are likely to attribute the temporary annoyance to the use of the ophthalmoscope, and declare how detrimental it has been to them. Any increase of disease is often set down to the same cause.

It is not generally known, indeed it is my own observation, that, when a strong solution of atropine is used to the one eye, the vision of the other is apt to be impaired for one, two, three, or even more days, and that without any apparent alteration in the pupil. The power of adjustment seems to suffer particularly. I shall not stop to discuss the physiology of this; I think it sufficient to mention the fact.

These are reasons why strong solutions should be avoided; and weak ones answer our purpose quite well. I have seen symptoms from the strong applications that leave me in no doubt that they were those of belladonna poisoning. In one instance I believe that death was so caused. The physician who was called to the young man mentioned his suspicions of such, without knowing that the drug had been used. A six-ounce bottle of lotion, containing thirty-six grains of the sulphate of atropine, was ordered as an eye-lotion, with directions for the wash to be constantly applied to the eye. These were fully carried out. The eye was kept saturated with wetted lint; several bottles were so used. What a large quantity must have passed into the throat through the lacrymal conduits!

Under very exceptional circumstances only therefore, and which the sagacity of the surgeon must determine, should a strong solution be used, and then only in small quantities. Half a grain of the sulphate to an ounce of water is what I ordinarily employ. A single drop, put on the conjunctiva, suffices. I depress the lower eyelid, and with a brush, my finger, or any convenient thing, apply the drop. Dilatation is much slower, but it passes away quickly, and the disagreeables incidental to the stronger form do not appear. Dr. Garrod has shown by his experiments that dilatation may be obtained by a grain in several ounces of water, but it must be remembered that healthy eyes are easier influenced than diseased ones.

It has been asserted that the retina is likely to become congested by the atropine. I have not been able to confirm this statement; and it is difficult to reconcile such a statement with the observation

of my colleague, Mr. Taylor, that the eye is more tolerant to the ophthalmoscopic examination when atropine is used, than otherwise.

[In a subsequent paper, Mr. Walton proceeds to diseased states of the eye, first noticing cataract.]

Cataract.—Few surgeons have long been engaged in active duty without being aware of the difficulty attendant on the diagnosis of incipient cataract. Nor is it the general surgeon only who may be so puzzled. From my own personal knowledge, such men as Tyrrell and Dalrymple were deceived, as the progress of cases shewed. It would be easy for me to adduce parallel examples occurring among living practitioners. Indeed, till the ophthalmoscope appeared, we were necessarily often in difficulty; and no one felt this more than myself, as the means at our disposal—the catoptric test and the concentrated light—were not enough. It was especially so in old age, when the natural coloration of the lens, the amber hue existed, and particularly when it was unusually developed, for then streaks of opacity at the back of the crystalline lens, or general haziness of this body, were easily overlooked. The coloration itself was liable to be mistaken for disease, when there co-existed impaired vision from any cause.

A wine-merchant was assured by a gentleman engaged in ophthalmic practice in this metropolis, that he had cataract. An operation was proposed, and consented to. The chloroformist was even named. I was consulted, in order to satisfy some friends. I found the lenses very highly coloured, but not opaque, for I could see distinctly all the fundus of the eye. Much hepatic derangement with a very debilitated system seemed to be the cause of defective sight, and a few weeks' treatment confirmed the diagnosis I had given.

Whenever, therefore, there is a suspicion of cataract, an ophthalmic investigation should be instituted. A less intense light than ordinarily employed, is preferable, because it is possible here to over-illuminate the eye, and the striæ may not be seen. With very high illumination, opacities may be less apparent than without the ophthalmoscope. It is on this account too, that the eye should be examined at first without the lens. A source of fallacy must here be guarded against. The dots of pigment that are so often seen on the capsule of the lens as the result of inflammatory attacks, by which the iris and the capsule have been brought into apposition, as also cicatrices or spots on the cornea, must not be mistaken for cataract opacities. It is enough to direct attention to this point to prevent that mistake. Positively, cataract may now be detected before a person is himself aware of it.

A lady applied to me for defective sight in the left eye. I had little doubt that I saw radiating striæ in her lens; but to be quite sure, I used the ophthalmoscope, and had the fullest evidence of cataract. I looked at the other eye, and discovered a few small and irregular linear opacities, which I had not seen in my naked eye

examination. I was told, in answer to my questions, that that eye was quite well, and vision was not impaired; yet I found by some delicate visual tests, that there was not perfect sight, but the derangement was so trifling, that it had not been noticed. I thought this a great triumph of modern science.

It is of no small diagnostic value to have a correct idea of the condition of the interior of the eye in cataract, to ascertain whether the loss of sight is in proportion to the degree of the opacity, and, in fact, depends on it, or on a more extended diseased state. We can get this knowledge by no other means. Do not considerations like these warrant an often repeated assertion of mine, that for what we have gained in this respect alone, the ophthalmoscope is of immense value; and if we went no further, might be truthfully considered amongst the greatest additions to recent ophthalmology.

Another condition of the crystalline lens can be learned only in the recent state by illumination: I mean, *Partial Dislocation of this body backwards, or twisting out of its place by direct violence.* This may be palpable later when the opacity has pervaded the lens, but certainly not by any ordinary means till then.

Disease of the Vitreous Humour seems to cause more astonishment to an inexperienced observer than any other morbid changes; partly, I suppose, because any physical alteration apart from greater fluidity was never supposed, and because the moving about of shreds and granules, and flakes and threads, are such palpable and material things. We now know that this body, which certainly is of low organisation, since it is often very rudely treated in certain operations, and portions of it are lost with apparent impunity, does become materially altered, partaking of disease that invades the eyeball generally. Apparently, at times, it is the sole seat of morbid phenomena, which are great impediments to vision. This is quite new to us; and no speculation, guessing, conjecture, or indication, brought it within the range of probability. In inflammations of the eyeball in general, it mostly becomes opaque, and looks muddy, and more or less hides, as in a fog, the fundus of the eye. Careful examination generally discovers minute bodies in constant motion. I have seen opacity of it, in the greatest degree, without any other symptom that I could detect, or any cause; and some cases that seemed quite hopeless, from the degree of turbidity, and the corresponding loss of sight, and the absence of all indication for treatment, have got quite well. So frequently have I seen the cloudiness pass away entirely in acute specific inflammations of syphilis and rheumatism, even where there were floating patches and filaments, which we call exudations, that I have little prognostic dread of it, as compared with cloudiness unaccompanied by acute disease.

Floating objects are not uncommon in the vitreous humour while its transparency remains; and they may exist alone or in connection with diseases of the retina or the choroid coats. These are generally

seen by the individuals themselves, and are often the circumstances that induce them to apply for relief. This brings me to speak of muscæ volitantes, in connection with the internal ocular inspection, and that only. I think that any one who has studied the writings of Sir D. Brewster, and the masterly essay of Dr. Mackenzie, himself a sufferer from muscæ, or investigated the subject with pains and care, cannot fail to conclude that they are minute floating bodies in the eye that cast their shadows on the retina, and that they are far back in the posterior part of the eye, almost in contact with the retina. As a consequence, this upsets the theory of pressure on the retina by varicose vessels causing them; but it does not invalidate the supposition that certain parts of the retina may be so pressed on, and rendered insensible, causing the loss of function to be manifested in fixed spots, or black disc that have not the motion, and other characters of muscæ.

I have not, so far as I remember, been able to detect moving bodies in the vitreous humour; but, in connection with other internal disease of the eyeball or defect of sight, I suspect that ordinary muscæ are too minute to be seen with the ophthalmoscope. It is becoming a prevalent idea that they may always be found if looked for. It is a fact which cannot be accounted for, that floating bodies in the vitreous humour are not always seen by a person, and that the perception of them varies.

Effusion of Blood in the Vitreous Humour is another newly recognized state. It is most threatening when extensive, because vision is quite obscured, and the eye to all appearance lost. It is nearly always impossible to trace the source; but, I presume, it must generally be from retinal vessels. It may be traumatic or idiopathic. The first is the rarer, and the amount of hemorrhage bears no direct relation to the severity of the accident; a mere tap on the eyeball may produce as much internal bleeding as a smart stroke; and I shall presently show that a blow about the region of the eye only, may cause intra-ocular hemorrhage. The second, the idiopathic variety, may, like the first, be unaccompanied by any external sign whatever; indeed, only now and then is there associated redness of the surface of the eyeball. The subjective symptoms, by no means always present, are flashes, corruscations, combinations of colour, and spectra. The actual condition, therefore, the diagnosis, cannot be known, except by the ophthalmoscope. In partial effusion the clot may be seen in the always more or less hazy vitreous humour. But the whole of the interior of the eye may be so darkened that no illumination can be obtained. I do not know how much blood is needed for this. Among the idiopathic cases the worst have occurred in eyeballs long subject to chronic inflammation, and the hemorrhage has quite destroyed the sight. Two of my patients lost their eyes in this way in the night. I have known recovery ensue many times when there has been so much effused, that I could not illuminate the eye; and I do not give a decidedly unfavourable prognosis, except there preexisted disease of the eyeball.

In the first case that I met with, I suspected the eye to be lost, but complete recovery ensued. A gentleman in good health, and with, as he thought, perfect eyes, perceived large soot-like bodies floating before the left eye. There was occasionally coloured clouds. He at once came to me. In a cloudy vitreous humour I saw the extravasation. There was no other objective symptom. I saw him two days after, and the vitreous humour was quite obscured. At the end of five weeks he could read any type, but was a little interrupted by some floating shreds in the vitreous humour, temporarily obscuring the type. A few weeks later his sight was quite restored, and I could not detect any abnormal ophthalmoscopic appearance whatever.

I will give the particulars of another case that occurred in the practice of my colleague, Mr. Taylor, and closely watched by him. Through his kindness, I saw the patient twice.

Mr. C., an artist, aged 30, applied to me for advice last February. A few mornings previously, on commencing work, he found that the sight of the right eye was obscured, to use his own words, "by various forms of dense matter, like those produced by blood dropped in water, or wreaths of blood-coloured smoke, the denser parts of which, when looked at against a strong light, appeared of a brilliant brown red." About two-thirds of the field of vision were totally obscured, the lower third being of a pale green colour, through which he could discern objects "with tolerable distinctness." During the following two days, the obscuration extended over almost the whole field of vision, and the colour of the wreaths deepened to black centrally. His written description was accompanied by a beautifully executed water-colour drawing of the spectrum. Though his health is generally pretty good, yet at the time his strength was rather reduced by over-work at his profession; he had had several slight attacks of bleeding at the nose, and had a bad cold and cough. The external appearance of the eye was perfectly natural. With the ophthalmoscope it was impossible to distinguish the fundus of the eye, the vitreous humour being of a dense grey or smoke colour; here and there only a faint red tinge could be perceived. As the eye moved, numerous small spots and filaments were seen floating across the pupil. In about three weeks the optic disc could be seen dimly, as through a fog; soon after, the retinal vessels could be distinctly traced, and a small dark red spot, the source of the hemorrhage, was seen on one side. The improvement was now rapid; and a little more than two months from the seizure, the sight of the eye was completely restored, though it was apt to become dim after a few hours' work; and a few floating filaments could still be seen in the vitreous humour with the ophthalmoscope.

Perhaps the commonest morbid change in the retina is *Hyperæmia*. The colour of the fundus of the eye is changed, being more red, and sometimes of a dark red. A portion of the optic disc is obscured by vessels, or the whole is covered thinly, so that the disc is yet made

out, or thick enough almost to obscure it, and then its position may be more readily traced by the vessels as they pass outwards, but which soon lose their distinctness.

This is a state that always exists in a greater or lesser degree with the train of symptoms called by Dr. Mackenzie "asthenopia," and supposed by him to be a defect of adjustment only. His reasoning is clear and powerful: his conclusion is inevitable, and, but for the ophthalmoscope, every one must have been satisfied with it. It is worth referring to as an example of the fallacy that we may fall into by attempting to solve such subjects by reasoning alone. I may recall the disease to the minds of my hearers by telling them that "asthenopia" is the "choroiditis" of Tyrrell. It is the affection of those who for the most part get their living by minute sight. They are obliged to discontinue with their work, because the vision as it were gets fatigued, and the objects confused or indistinct, and vanish from view altogether. A slight cessation, a wink of the eyes, a look at a distinct object, or, in the worst cases, a pause for a minute or two, enables them to continue. It is strange that the indistinctness comes on when the vision is required to be sustained on minute things only, and not when large and distant objects are looked at. But it is not peculiar to the overworked eye. I have seen it in children learning to read. in the idle man of pleasure, and in the vigorous mechanic. I begin to think that it is almost a defect of organisation, rendered worse or apparent under much or peculiar use of the eye, and the general inutility of treatment, short of cessation of that employment which renders it annoying, tends to this conviction. I could fain dwell longer on it.

Among other ophthalmic revelations of retinal changes, are those of *dots*, or *patches of white*, usually seen in the aged, and associated with feeble vision. I do not know that their nature has been made out.

Œdema of the retina, rendering the membrane pale and of a yellowish tinge, less shining than in health, and apparently raised around the optic disc, is fully recognised.

Pigment Deposit on the retina, in every form of spot, and in extent from a few dots to almost complete covering, with but small interspaces, is the most remarkable change in this tunic, and not less is the absence of any coexisting external symptoms. I have seen an amount of deposit that seems incompatible with any vision on first examination of a patient for slight feebleness of vision, which is, of course, a physiological puzzle. Many times I have doubted the visual power till I have carefully tested it.

Choroid.—The choroid, like the retina, becomes *hyperæmic;* but the most striking change is *Atrophy*, chiefly manifested by the removal of its pigment; a common consequence of chronic inflammation. The alteration generally begins near the optic disc. At first a yellowish change is seen, and then the orange-coloured choroidal vessels appear, and these become more distinct, ultimately disappear, and

leave a white spot, which is often mistaken by the inexperienced for the optic disc, especially if the disc be injected. With the loss of pigment, there is a deposit of grey or deep black spots in other parts, as if the colouring matter was removed from one portion and taken to another. Associated with this, extravasated blood, in small quantities, is not very unfrequent, so that the whole fundus of the eye has a mottled and variegated appearance, and withal a person may be able to read. These alterations rarely exist without some disease of the retina.

Early in my professional career, indeed in my studentship, I suspected that in so-called *Syphilitic Iritis* there was something more than the name at first led me to infer, and this chiefly on account of the subsequent damage, the very defective eye that remained after the iris had recovered. I was particularly attracted to this in a young woman, during my dressership. The eye seemed perfect when the inflammation was passed, yet she could hardly count my fingers. Well do I remember this being attributed by the gentleman, whose patient she was, to deficiency of active treatment in the first instance, a remark which explained nothing. The evidence of implication of the eyeball by several local and general symptoms were unheeded by me, for I looked on them, as I was taught, merely as the consequences of an inflamed iris, that being the part which the disease primarily attacked, and on which it spent its violence. It was with inexpressible delight, some years after, that I read Dr. Jacob's most valuable monograph on inflammation of the eyeball, and became convinced by it that iritis, so called, was not disease of the iris alone, but general inflammation of the eyeball; and, indeed, who ever saw the iris discoloured and paralysed by inflammation from syphilis, or rheumatism, or struma, without vision being materially impaired? I have ever found that Dr. Jacob's writings stand the test of time, and are less eclipsed by the advancement of knowledge than those of any modern ophthalmic author. Ophthalmoscopic examination does not render them obsolete, but adds confirmation.

It is no longer a secret why syphilitic eyes become spoiled. But with faint implication of the iris, and little symptom of redness and pain, the retina may be spoiled, as we see by the deposit of lymph, and choroidal damage is subjoined by structural alterations. I once gave a wrong prognosis in a case of rheumatic inflammation of the eyeball, that annoyed me for a long time, and created a great desire to discover the cause of an eyeball being so insidiously lost. With but very slight external evidence of implication of the iris, I gave assurance of complete recovery; but, to the great disappointment of all, blindness ensued. This, together with ophthalmoscopic experience, has taught me always to give a guarded opinion, and I may here remark, with regard to prognosis in general, that most of us are too apt, in critical cases, at a great risk to our reputation and comfort, and without any adequate advantage to ourselves, to be too prophetic,

and to promise too much. It is true that patients almost force us into it, and when we commit ourselves, it is our desire for a favourable issue that betrays us.

A more extended knowledge of syphilitic disease of the eye, has convinced me that the posterior part of the eye, certainly the retina and the choroid, and perhaps also the vitreous humour (which is indicated by haziness and flocculi), are always involved. I never saw an example without impaired vision, and all observers know that vision may become misty prior to any palpable iritis. Before I ever looked through an ophthalmoscope, I made the vision the chief ground of diagnosis in the incipient stage of the disease. Many a case of reddened eyeball presents itself, and looks so like iritis, that the most experienced observer cannot say at once that there is or is not this affection; but the test that never fails me is trying the seeing power, and if I find no deficiency here, I am sure that there is only surface inflammation.

A surgeon in very large practice, in London, and well known to all of us, sent very lately for me to see a patient whom he had attended with rheumatism, and now, as he supposed, afflicted with rheumatic iritis. The eyeball was much injected, and whether superficially or not, I could not tell. This iris was clear. I tried the sight, and when I found that the almost microscopic inscription on my watch could be read, I was satisfied that there was no iritis—no "eyeball-itis". The calomel and opium pill, which was just commenced, was left off, the usual diet was allowed, and an astringent lotion given. In a few days the eyes were well.

More than all this, the ophthalmoscope teaches us that the retina may be the sole seat of syphilitic action. My attention was first drawn to this in a medical pupil saturated with secondary syphilis, and who lost the sight of one eye without any external symptoms. Internal inspection, however, showed that there was inflammatory exudation over the fundus of the eye, obscuring the optic disc and retinal vessels. When I was last consulted, fortunately for him, his sight was almost clear.

Among the appearances at the fundus of the eye, after the acute stage of syphilitic eyeballitis has passed away, are dots and patches of deposit of different forms and sizes, more or less darkened with pigment, over the retina. The optic disc is often changed, sometimes contracted and ill defined, and greyish; sometimes, apparently, more spread out, and presenting many abnormal aspects. The choroid becomes patchy with white, yellow, and brown; its pigment is lost in spots, allowing the vessels to be seen, and exposing the sclerotic coat.

The ophthalmoscope has not increased the difficulties of ophthalmic practice, on the contrary, it renders it easier, after a man has taken the requisite trouble to learn certain preliminaries. What time used not to be spent in investigating cases of impaired vision, to discover

to which of the subdivisions of amaurosis it belonged? How numerous the questions, how tedious and unsatisfactory the examination? Now we at once have a peep, and see things as they are, and when we are puzzled, the patient is not made to suffer for it. I believe, too, that the ophthalmoscope will reduce ophthalmic literature; the treatises will devote less space to the diseases of vision, because less is needed. There will be more fact, which is always put in a small space, and less of wordy theory.

There is just one topic, respecting therapeutics, that I venture to introduce. Do not abandon a patient, and say that he is incurable, merely because you see this or that change in the eye; it is one of the dangers of a speciality. Let nothing short of disorganisation prevent you from seeking indications of treatment, and catch at the shadow of a chance. We know so little of the natural reparative power, of function and of vitality, that we ought not to dare, except under remarkable and well recognised conditions, to give a fatal prognosis.

Do not look on a diseased eye merely as such; remember that this organ is but a part of a whole, and that recovery, or partial recovery, often ensues contrary to the expectation of the most experienced. Which of us has not seen this exemplified in disease of the lungs, and kidneys, and liver, by specialists in these departments?—*British Med. Journal, June* 16, *August* 4 *and* 18, 1860, *pp.* 452, 597, 643.

91.—*On the Surgical Treatment of Short Sight.*—Mr. J. V. Solomon, of the Birmingham Eye Infirmary, has hit upon an operation by which the focal range of short-sighted persons, whose cornea are not conical, may be doubled in length. He has found the plan equally successful, whether the eyes are prominent or small, the aqueous chambers deep or shallow. He has tested the operation on cases varying from the age of 12 to 45 years. A man of the latter age, who had worn double concave glasses of immense depth (No. 16) for a great number of years, and, unaided by lenses, could see with clearness the features of a person to know them at a distance of nine feet only, obtained at once by the operation an increase of seven feet in his focal range. In a child of 12 years of age, the operation increased the reading distance from to four to eight inches, and the power of identifying persons' features from twenty to forty yards; and in one of 16 years, the effect was still more remarkable.

These results have been obtained by dividing in a *transverse* direction some fibres of the muscle of the lens—the ciliary muscle. Mr. Solomon does not consider it material which part of the muscle is selected for division, but generally prefers either the upper or the lower part of the circle. Supposing the latter situation to be selected, and the patient to be seated in a chair, the operator stands behind, and fixes the globe with the left fingers, as in extraction, holding a

cataract-knife in his right hand; with the flat of the blade directed *upwards*, he pushes the point in succession through the corneo-sclerotic union, the pillars of the iris, and the ciliary muscle. The direction given to the instrument is obliquely downwards and outwards. Care is taken that the incision in the muscle is of the same length as the puncture of entrance; namely, about two or two lines and a half in diameter. In some cases, Mr. Solomon has found that the power of adapting the eye to distant objects has been increased by practice and lapse of time.

In a young man who had been myopic from his childhood, and had suffered for the last three years from congestion of the retina, the visual power and focus have been so much increased that the outline of large buildings at a distance of a mile and a half can now (six weeks after operation) be distinctly made out; whereas, before the ciliary muscles were divided, they appeared as mist.—*British Medical Journal, May* 26, 1860, *p.* 393.

92.—*Rheumatic Ophthalmia treated by the Galvanic Current.* By HARRY LOBB, Esq., M.R.C.S.—R. J., had suffered for more than fifteen years from repeated attacks of rheumatic ophthalmia, through which the pupil of one eye had become permanently contracted, and the sight of the other much injured. Being warned that a repetition of these attacks would be followed by total loss of sight, he was led to consider whether some means of prevention hitherto untried might not exist. He had found by experience that galvanism applied to the limbs removed rheumatic pains; he therefore thought that a modified current applied to the eye at the commencement of an attack might prevent its occurrence, and if applied for a sufficient length of time the cure might be permanent.

On the approach of the next attack, he accordingly applied an excited thirty-six link Pulvermacher chain, the positive pole upon the spine, the negative upon the closed eyelid, upon retiring to rest. This was allowed to remain on during the night. In the morning, every trace of the disorder had vanished.

Other attacks followed at gradually increasing intervals of time, but they were invariably arrested by the same means, the consequence being, that during the last five years R. J., has found that his eyes have been gaining strength, enjoying the uninterrupted use of his sight during the whole time.

The above case, communicated by the patient himself, requires no comment from me, as it is related in so plain and straightforward a manner. At the same time I may corroborate it by stating, that in all cases of ophthalmia for which I have used the continuous galvanic current a rapid cure has been effected.—*Lancet, Sept.* 1, 1860, *p.* 210.

93.—*A Simple Mode of treating a frequent form of Entropion.* By M. SICHEL.—There are varieties of this affection which only yield to appropriate operative procedures, but there is one form which almost always precedes the others, and which can be relieved in the great majority of cases by very simple means. It is well known that in ophthalmia accompanied by violent photophobia, and by great distension of the vessels of the conjunctiva, the patients insensibly acquire the habit, especially when the inflammation has become chronic, of of strongly contracting the eyelids, with the double aim of excluding the rays of light and of expelling the foreign bodies which, from their sensations, they believe to be present at the front of the eye. This constant contraction of the orbicularis at last inverts the free edge of the lower eyelid, the very narrow tarsus of which yields more readily to the blepharospasm than that of the upper eyelid, which has a more considerable vertical diameter. After a while the cartilage of the lower lid can no longer resist this incessant action, and it undergoes an incurvation which becomes more and more considerable, until at last complete inversion is produced. When the affection has attained this degree, the means now to be mentioned are usually of no avail.

The entropion is confined in most cases to the lower eyelid. It is most frequently so after chronic ophthalmia and the operation for cataract, when the patient, either from indisposition to open them or from excessive sensibility of the retina, keeps the eyelids too long closed. In this variety of entropion, termed spastic, the advice is usually given to practise frequent tractions on the lower eyelid, or to keep it everted on the cheek by means of adhesive strips. In the author's hands these means, powerless against the permanent contraction of the orbicularis, have always failed, while a very simple manœuvre has generally succeeded. In the place of producing complete abduction and depression of the eyelid, it should be only moderately depressed, and stretched vertically by means of the index and medius fingers, so as to sensibly separate it from the anterior surface of the eye. The pulp of one of the fingers is next applied above the inferior edge of the orbit, upon the adherent part of the eyelid, and gently carried from before backwards into the anterior portion of the cavity, until the free edge of the eyelid is strongly everted. The pressure must be made very gently, the finger sliding along the anterior portion of the floor of the orbit without compressing or irritating the globe. This simple manœuvre, repeated every quarter of an hour or oftener by the patient himself, leads to the re-establishment of the eyelid in its normal position. The same procedure, with some modification, may be advantageously employed for the treatment of entropion of the upper eyelid—an affection which, indeed, is much more rare, and infinitely more difficult to cure.—*British and Foreign Medico-Chirurgical Review, Oct.* 1860, *p.* 544.

MIDWIFERY,

AND THE DISEASES OF WOMEN, ETC.

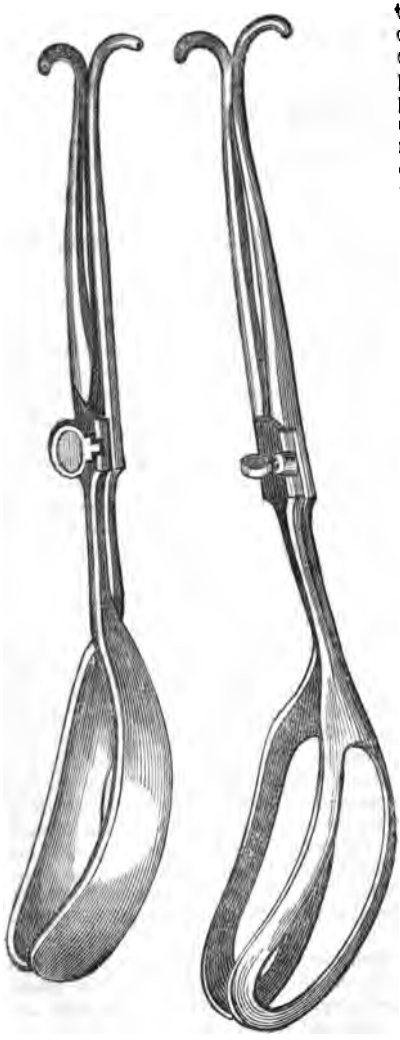
94.—NEW PATTERN FOR THE OBSTETRICAL FORCEPS.

By Dr. Samuel T. Knight, Baltimore.

[Dr. Smellie's short forceps have been in common use in America and England for many years, modified by various practitioners. Dr. Davis's forceps are nothing more than one of these modifications. Dr. Knight's forceps have a lesser pelvic curve, have no fenestra, have smaller blades, taper more gradually from the handles to the blades, and the blades are more convex, especially near their termination; and, finally, the blades are more narrow, rendering their introduction simpler. All these seem to us to be great advantages. We consider that the forceps, if made to be introduced more easily, especially the upper blade, and if made to fit the head better, might be much more frequently used. These forceps of Dr. Knight's are the best kind we have seen engraved, and we only wish he had sent us a pair from America, that we might have shown them to our friends.]

The objections to Davis' or Hodge's forceps (as they are variously termed) offered for sale by the instrument makers, and the only ones now kept as stock on hand by them, are—1st. The pelvic curve is greater than necessary, according to the laws of mechanics. 2nd. The suddenness of the curve, with the fenestra beyond, demands too great an amount of metal at the point of curvature. This is the precise spot where the greatest volume of the head is brought in contact with the perineum, when these instruments are used in extracting, and by this abrupt tension endangers, one might almost say insures, in unskillful hands, the rupture of this part. 3rd. The large size of the fenestra allows too free a protrusion of the fœtal cranium through them. By this bulging of the cranium, the biparietal diameter of the fœtal head is but little reduced. 4th. The fenestra increases the breadth of the blades more than is convenient or necessary, particularly in many pathological conditions for which the forceps are especially applicable.

Those who have constructed the forceps, by taking a cast of the curve of the bones of the pelvis denuded of the soft parts, have seized a physiological, but not a pathological, idea. The difference of the axis of the outlets of the recent and of the bony pelvis is known to all obstetrical students. Nothing can be more false, therefore, than



to adapt the pelvic curve of the forceps to the axis of the cavity of the bony pelvis and its outlet. The position of the patient during the operation must also be taken into consideration. Were it required to operate whilst the patient was in horizontal position, stretched upon the plane of a bed, the pelvic curvature of the forceps would have to be increased. But, in the proper position for the application, the depression of the handles of the instrument easily compensates for the curve in the blades. It must be granted that the straighter the blades, the greater the mechanical advantage and the increased certainty of action. This is particularly manifest at the point of outlet over the perineum. When the curvature of the blades is great and sudden, a sufficient elevation of the handles cannot be obtained by the operator with proper mechanical effect. The danger to the perineum, from the curvature of the blades and their sudden springing from the handles, is not the only objection to the physiological forceps of the learned Professor of the University of Pennsylvania. The fenestra are not only useless, but

injurious. They weaken the blades and render them more likely to slip, and, when they slip, they tear and mutilate the fœtal scalp. Moreover, the protrusion of the soft parts through them increases the resistance more than the polished metallic surfaces of the plain blades. When the forceps with plain blades, according to the measurements given, are properly applied, it is impossible for them to slip. They grasp the head accurately, and the strength of the blades makes good their position. The proper force to be used is, therefore, more easily estimated by and is entirely under the control of the operator. The fenestra weaken the blades, rendering them, to a certain degree, flexible and elastic, oftentimes yielding before a resistance, both justifiable and necessary to accomplish the end in view.

The breadth of the blade of the plain forceps and the moulded convexity of their interior face, which adapt them accurately to the arch of the cranium, entirely prevents any cutting or mutilating of the fœtal scalp. In no operation, when they have been used, has the print of their application been left. In the gradual taper from the handles to the blades, they approach more nearly to Dr. Smellie's forceps, and at this point give a decided advantage.

Measurements of Hodge's Forceps.	*Measurements of Knight's Forceps.*
Whole length, 16 inches.	Whole length, 14½ inches.
Weight, 1 lb. 1 oz.	Weight, 16 oz.
From joint to end of handle, 6·8.	From joint to end of handle, 6¾ in.
Length of parallel shank, 3·5.	Length of parallel shank, 3.
Length of proper blade, direct line, 6.	Length of proper blade, direct line, 6.
Breadth of blade at broadest part, 1·8.	Breadth of blade at broadest part, 1⅝.
Perpendicular elevation of points of blades where the instrument is laid on a horizontal surface, is 3·4, which indicates the degree of curvature of the blades.	Perpendicular elevation of points of blades where the instrument is laid on a horizontal surface, is 2⅝, which indicates the degree of curvature of the blades.
Breadth of fenestra, 1·1.	No fenestra.

"Hundreds of modifications," says Cazeaux, "have been proposed in the forceps since the days of Levret and Smellie, nearly all of which have fallen into oblivion. Some of them were quite ingenious, but they imperfectly attained the end their authors had in view, and others were nearly destitute of value or utility." The present article would not have been penned were it not for the honest conviction, obtained by a sad experience, that it were better for suffering females that those now in common use throughout this country should *also fall into oblivion.* The particular faults in their construction have been pointed out, and it is to remedy these that the present instrument is offered.—*Maryland and Virginia Medical Journal, April* 1860, *p.* 272.

95.—THE FORCEPS AND PERFORATOR COMPARED.

By Dr. CHARLES DAVID DOIG, Louth.

Towards the close of last year an article was contributed by me, and printed in the 'Medical Times and Gazette,' on the subject of Cross Presentations. The principal points illustrated in that paper were the following:—

1. That in the event of cross presentation of an average-sized fœtus in an average-sized female with a normal pubis, the result to be anticipated is rupture of the uterus and death of the mother by exhaustion.

2. That laceration of the uterus may also occur in the course of a natural labour.

3. That if the mother be not deformed, and have an average-sized pelvis, while on the other hand the contained fœtus is comparatively small, as in premature births, or abortions, the mother may be delivered in safety by the natural powers.

4. That podalic version is an efficient means of delivery as far as the life of the mother is concerned, and not necessarily fatal to the life of the child.

5. That the operation should be performed as soon as a transverse presentation is diagnosed, and the vaginal and uterine passages permit the introduction of the hand.

6. That in cases of version from all causes at the ninth month, one mother in fourteen is lost, and sixty per cent. of the children still-born.

It is unnecessary to carry these inferences further; I shall therefore briefly consider the treatment applicable to another class of difficult labours, which forms the immediate object of this article.

One of the diseases liable to be developed in the course of protracted parturition is vesico-vaginal fistula. Mr. I. B. Brown, who has conducted the treatment of twenty-six cases of this disease, and published minute details thereof, maintains as the result of his experience and observation, that vesico-vaginal fistula had in almost every instance been consequent on protracted pressure of the child's head on the mother's vagina, and strongly recommends obstetricians not to sit days and nights with women in labour, but at once afford to the patient the aid adapted to the circumstances of the case. "Dr. Veit" shows, "1. That the danger for the child when the birth is completed within twelve hours is only half as great as when the labour is protracted to twenty-four hours, and that further protraction is still more dangerous. 2. The danger is increased when the second stage of labour lasts longer than two hours. 3. The male sex is more endangered than the female." Protracted labour may then not only be the means of impeding the perfect recovery of the mother by originating dangerous complications, but also of endangering the life of the child.

Delivery by the forceps and perforator are long established obstetric operations. The following cases indicate the principal distinguishing feature of these two methods of delivery. The first does not of necessity destroy the life of the child; by the second the life of the child is intentionally sacrificed.

Case 1.—In 1852 I was requested to visit a female in lingering labour. She mentioned to me that she had been very ill all night. On examination I ascertained the passages to be well dilated, the presentation cephalic, and the existence of no impediment from retained urine and fæces. After waiting several hours to assure myself that the natural powers were acting inefficiently, I called on a medical friend (Dr. Gillespie, Leith) to get the loan of a pair of long forceps, as I had not a proper pair by me. We visited the patient together, and extracted the child instrumentally. The child was alive. The mother made a good recovery. Low diet, a dose of opium, and a gentle aperient, were all the remedies adopted in the after-treatment of the case.

Case 2.—In October, 1854, I was requested to attend a patient at her confinement. Mrs. C. was a primapara, about 30 years of age, and resident at St. Mary's-place, near Wapping. I sat up all night with the lady, and towards morning, getting weary of the charge, determined to extract by the forceps if such could be accomplished. She had made no progress, and there was no difficulty in satisfying myself as to all the points of the presentation. However, the space left for the application of this instrument was so slight in consequence of the large size of the fœtal head, and the swollen state of the surrounding parts, that the blades could not be introduced; I determined, therefore, to consult with Dr. Lever, of London, before performing craniotomy. It was his opinion that the use of the forceps was impracticable, and perforation imperatively demanded. We accordingly anæsthetised the lady, introduced the scissors, broke up the nervous matter of the child, and extracted. The mother recovered well with very simple treatment. She was put on low diet, took small doses of calomel and antimony, one or two aperient senna draughts, and tonics.

Dr. Lever was of opinion that the child had been dead some days, which removed any qualms of conscience that we might have had, had we been breaking up the cerebro-spinal axis of a mature living child.

In these, as in all other cases, it has been my custom to apply a binder after delivery. It is important always to have the attention directed to the state of the rectum and bladder, as accumulation of urine or fæces retards parturition, and their removal by the catheter or enema further delivery. No such obstacle occurred in these cases.

The long forceps is now in more frequent use than the short. The instrument consists of two doubly curved fenestrated blades, variously shaped, and constructed according to the taste and judgement of the Practitioner. The necessity for their use being determined, the patient

is placed on her left side at the edge of the bed, the passages lubricated if necessary, and the forceps warmed and greased. The blade is gently insinuated with its concavity applied to the convexity of the child's head, the upper or anterior blade being applied first, then the inferior blade. If the forceps do not lock, a blade must be withdrawn and re-applied so that this object is attained.

The forceps are efficacious partially by compressing the fœtal head, but more particularly by a kind of undulatory traction which the operator should exert in the direction of the axis of the pelvis during uterine action, or if this be wanting, at intervals. In using such force as is needful to change the position of the head, great care is requisite to avoid injuring the soft parts of the mother, or lacerating the perineum.

The instruments required for the performance of craniotomy are a pair of scissors with shoulder-stops, a crotchet, and a pair of bone forceps. Besides these there are many other weapons that are found of service in particular cases. The scissors or perforator have sharp outer margins. Either should be introduced by the right hand, the left hand being used as a guide and guard, through a parietal bone and not through a suture, pushed into the skull and moved about *in* such a way as completely to break up the brain and medulla oblongata. The force should be applied at right angles to the parietal bone, to prevent the instrument slipping and tearing up the scalp or *doing* other injury. The crochet is then used to complete the operation. It may be used to tear up the brain as well as to extract. It can be fixed on the parietal bone, on the occipital, or on the foramen ovale. Before using strong extraction force with the crotchet the firmness of the hold should be tested. The craniotomy forceps become useful in many instances as a means of applying traction to the child. It is a rule only to work during the pains.

Such are the formidable instruments that were required in the treatment of the two cases detailed. We come next to consider what are the cases in which the forceps should be applied, and what are those suitable for the perforator. Dr. Davis used the forceps *six* times among 7302 deliveries:

1. Three were uncomplicated head cases.
2. Two were first children in twin cases.
3. One was puerperal convulsions.

Dr. Ricker, of Nassau, mentions the indications for the use of the forceps in a large number of instances which, tabulated, give the following results:—

1. Disproportion between child's head and mother's pelvis	287
2. Feebleness and absence of pains	269
3. Weakness, exhaustion, and illness of the mother (hernia, phthisis, &c.)	33
4. Prolapsus of the umbilical cord with the head	29
5. Spasmodic and preternaturally painful pains	22

6. Face presentations	20
7. Ecclampsia and convulsions	12
8. Prolapsus of smaller parts of child with head	8
9. Placenta prævia, 3; other hemorrhages 7; total	10
10. Oblique presentation of the head	7
11. Rigidity of the structure in elderly first-bearing women	4
12. Great swelling of soft parts	4
13. Erysipelas pudendorum	7
14. Putrescence and consequent unusual contractility of uterus	1
	713

Dr. Davis performed craniotomy nine times among 7302 deliveries:

1.	In consequence of deficient pelvic space	3 times.
2.	„ „ pelvic contraction caused by hip-joint disease	1
3.	„ „ carcinoma uteri	1
4.	„ „ patient aged 14½ years	1
5.	„ „ face directed to pelvis	1
6.	„ „ rigidity of os uteri	1

The preceding data indicate that deficient pelvic space is one common condition requiring operative procedure; in what cases, then, of pelvic contraction may the forceps be applied with success, and in what must the perforator be resorted to? It is difficult to come to a definite conclusion on this point, as each case requires to be considered individually. In the instance detailed, craniotomy was performed in consequence of disproportion between the child's head and the pelvis, attended by comparative feebleness of the pains. In many cases, however, requiring this operation, or the less serious one, the forceps, the pelvis is distorted, as well as of an unequal size. Although it is impossible to lay down rules applicable to every individual instance, we can come to conclusions, which admit of general application. The conjugate diameter of the normal pelvis varies from four inches at the brim, to four and a-half inches at the outlet. Dr. Simpson, of Edinburgh, used to lay down the following rules that the forceps could be applied if the conjugate diameter was three and a-quarter inches, and that embryulsio was to be adopted when the conjugate varied from one and three-quarter inches to three and a-quarter inches.

We come next to inquire what is the frequency of these operations compared with other obstetric operations, and with midwifery practice in general; and what is the mortality attending the operations. The results of the statistics of different authors are more various than is desirable, and are exhibited by the following figures:—

		Labours.	Forceps.	Proportion.	Craniotomy.	Proportion.
Dublin	Clarke	10,199	14	1 in 728	49	1 in 248
,,	Collins	16,654	27	617	118	141
Paris	Baudelocque	17,388	31	561	6	2898
,,	Lachapelle	22,243	76	293	12	1854
,,	Boivin	20,517	96	214	16	1282
Vienna	Bon	9589	35	274	13	737
Heidelberg	Naegele	1711	55	31	1	1711
Berlin	Kluge	1111	68	16	6	185
Dresden	Carus	2549	184	14	9	283
Berlin	Siebold	2093	300	7	1	2093
London	Davis	7302	6	1217	9	811
Nassau	Ricker	304,150	4223	72	165	1843

The following figures give more minute particulars with reference to the frequency of these two operations in British Midwifery —

Simpson	1	Forceps in 472	cases.	1	Craniotomy in 1417	cases.
Lever	1	,, 518	,,	1	,, 186	,,
Churchill	1	,, 546	,,	1	,, 149	,,
Ramsbotham	1	,, 611	,,	1	,, 805	,,
Collins	1	,, 617	,,	1	,, 141	,,

It may therefore be with safety inferred that in British Midwifery forceps cases occur once in 550 births, and craniotomy once in 530 births.

With regard to the mortality attending forceps cases, Dr. Ricker mentions that out of 4223 instances 93 women died either during, or soon after, the operation, and 684 children were still-born. Hence 1 woman out of $45\frac{38}{93}$ deliveries by the forceps died, and 1 child out of $6\frac{419}{684}$.

Dr. Ricker also states the results attending the breaking up of the fœtus, which may be arranged under two divisions:—

	No.	Proportion.	Lived.	Died.	No information.
Perforation	143	1 in 2196	88	35	20
Embryotomy	22	1,3625	16	6	

In the course of the preceding remarks have been considered—1. The mode of using the forceps; 2. The method of practising craniotomy; 3. The circumstances demanding the performance of these two operations; 4. The diameter of the pelvis that admits of the use of the forceps or the performance of craniotomy; 5. The comparative frequency of the two operations; 6. The success attending each. The investigation might be profitably carried to a much greater length, but the object of this paper has been rather to associate this statistical inquiry with actual cases, then enter into minute details.—*Med. Times and Gazette, July* 28, 1860, *p.* 74.

96.—ON A NEW METHOD OF VERSION IN ABNORMAL LABOUR.

By Dr. Braxton Hicks, F.L.S., Assistant Physician-Accoucheur at Guy's Hospital, and Physician to the Royal Maternity Charity.

[Several Continental writers have mentioned the possibility of version by external manipulation,—as Wigand, Mattei, and Stolz; and

Martin, of Jena, has related many cases in which he was successful in turning by external manipulation. All these writers have used external manipulation *only*.]

Before I had read their plans of operating, I had already employed a method, combining the power possessed by external manipulation with the power and certainty derived from pressure and touch through the cervix, and which, even since the perusal of them, appears to me more satisfactory, and more easily managed by those who have not given, nor are able to give, to the external diagnosis of fœtal position such care and skill as Prof. Carl Esterle; for even Martin, of Jena, insists that by his plan it is necessary—1st, that immediate delivery be not called for; 2nd, that there be a capacious pelvis; 3rd, that there be active pains; 4th, that the child be living. From the following remarks and illustrative cases, I think I shall show sufficiently that the method I have employed does not require such conditions. Accepting, then, the valuable fact, that the fœtus in utero can be turned without the necessity of the entry of the hand into the uterus, I shall now prove the extensive applicability of the plan I have adopted to labour in its various complications.

I do not consider it needful to stop here to prove the major proposition, because it will be immediately acknowledged, after a few trials on the plan described below. The points to which I wish therefore to draw the attention of the profession are—1st, the most certain method of operating; 2ndly, the general reasons of its superiority to other methods; and 3rdly, the cases to which it is more particularly, and also exclusively, adapted.

In considering the first point—viz., the most certain method of operating, it will be necessary to reflect upon the principal facts in regard to version. One of the most important, and one which more particularly bears upon the method presently to be alluded to, is, that if we place by any means in our power the fœtus in utero in a transverse direction and abdomen downwards, the knees of the fœtus are opposite the os uteri: and if the os is large enough to introduce a finger, the knee can be hooked down and the presentation secured. Thus it is evident that it is merely necessary for the fœtus to assume the above position, where delivery by knee or foot is sought for, or where we wish that the delivery may be at our command. I shall defer alluding to version of cephalic into pure breech presentation till I speak of the method of operation.

The next fact to be borne in mind is that, should the fœtus, either naturally or by the method to be described, present by its back to the os uteri, the case would be very rare in which one side or the other was not the more dependent, and consequently one or the other knee be within finger-reach of the os.

The third fact needful to reflect upon is this, that the shape of the cavity of the fully-developed uterus being oval, with the long axis

placed vertically, the tendency of the slight pains towards the end of pregnancy is to place the longer axis of the fœtus in a corresponding direction; or, in other terms, to cause it to present by the head or breech: so that if the fœtus be at any time situated transversely, a slight preponderance of force in either direction will be sufficient to determine the ultimate presentation. It will be thus perceived that, the head being heavier than the pelvic extremity, the natural tendency of a spontaneous rectification will be to head presentation; but at the same time it would have taken but little force to have caused it to rise to the fundus.

Having premised these considerations, we will discuss the other facts as they arise in the application of the above principles, taking for granted that it is admitted possible to turn the fœtus in utero from the outside. The method I have found successful, and very easy of application, is conducted thus:—We will suppose the simplest condition, a case where the uterus is passive, membranes unbroken, the liquor amnii plentiful, the os uteri expanded sufficiently to detect the presentation, which is cephalic, and in the first or fourth position (occiput to left side); the patient is in the ordinary position, the trunk curved forwards as much as possible, to relax the abdominal muscles. Introduce the left hand, with the usual precautions, into the vagina, so far as to fairly touch the fœtal head, even should it recede an inch. (This generally requires the whole hand.) Having passed one or two fingers (if only one, let it be the middle finger) within the cervix, and resting them on the head, place the *right* hand on the *left* side of the breech at the fundus uteri. Employ gentle pressure and slight impulsive movements on the fundus towards the right side, and simultaneously on the head towards the left iliac fossa. In a very short time it will be found that the head is rising and at the same time the breech is descending. The shoulder is now felt by the hand in place of the head, it in like manner is pushed to the left, and at the same time the breech is depressed to the right iliac fossa. The fœtus is now transverse; the knee will be opposite the os, and, the membranes being ruptured, it can be seized and brought into the vagina.

Having now the labour at command, the case must be treated according to the circumstances which call for turning. In obedience to the law above stated, when the fœtus is placed transversely, a slight impulse will determine the final position of the head. When the leg is seized, therefore, it is advisable to place the right hand beneath the head in its new position on the left side, and gently press it towards the fundus. The same law renders it very easy to convert the cephalic, shoulder, neck, or even natural transverse, into a breech case. In either of these conditions it is merely necessary to push up the head, and, removing the left hand from the vagina without bringing down the knee or foot, place it on the breech in the righ tiliac fossa, so as to depress it into the cavity of the pelvis. No extra force should be used, for it will be found to obey a very "gentle persuasion."

Indeed, the change is completed sometimes spontaneously, and the foot is at hand before it is expected. The breech now presenting, it is advisable to retain it till labour is fairly set in, by a firm bandage placed externally, and, should it be required, a pad on either side of the fundus. It should now be treated as an ordinary pelvic presentation.

We have in the above case supposed the fœtus to have been placed with the back to the left side (first and fourth positions). Should it, however, be certainly ascertained to be placed in the opposite direction (second and third positions), the only alteration required is to make the pressure and impulses in the opposite directions, following the same rules. But suppose the exact position cannot be made out: in that case it should be considered as being in the first and fourth, and treated as above directed; and for the following reasons:—1st. The fœtal head presents in that position (back to left side) in from sixty-five to eighty per cent.; consequently the practice would be correct in that proportion. 2nd. Should the child be turned over on its back, one side is almost certain to be the most dependent; consequently a knee or a foot will be within a finger's reach. 3rd. Should the lower limbs not be near, it can be readily converted into a breech case, whereby the foot or breech will be close to hand. Suppose, again, the liquor amnii has escaped—although it certainly does make the operation more difficult, still, as the cases to be quoted will show, it is easily managed even then, the principal impediment being the activity of the uterus; should that render it troublesome, suspend it for the moment, either by chloroform or opium, and I believe the escape of the waters will be found to be no objection to operation. With respect to other conditions which may retard or interfere with it, they must be treated in the same manner and on the same principles as apply to the ordinary method, and require the same judgment on the part of the operator.

In considering, secondly, the general advantages of this operation over the ordinary method, I disclaim all intention of unnecessarily depreciating an exceedingly valuable and ancient operation—one which has saved numberless lives, and one with which, at present, we cannot dispense. Still, if it can be shown, that in a considerable number of cases requiring version, the operation can be accomplished as quickly, or even more so, without the necessity of introducing the hand into the uterus, with the exception of one or two fingers passed a little way into the os, I am sure that such a modification of this more or less hazardous operation will recommend itself without any panegyric on my part. For in that case it will readily be perceived that we shall avoid—

1. The addition of the hand, and perhaps arm, to the uterine contents; and the irritation, present and future, caused by it.
2. Entry of air within the uterine cavity.
3. Liability to rupture of uterus.

4. Much of the pain and distress felt in the ordinary plan.

5. The removal of the coat, and baring the arm of the operator; and, as a minor consideration,—

6. The fatigue and pain endured by the operator while the hand is in utero.

I think the removal of these objections a matter of considerable importance, even if option were given us in all cases; but more especially so when we add to this the power the plan I have suggested gives us—namely, of being enabled to turn under circumstances totally impossible by the older method. I feel confident that all impartial practitioners will, after a fair trial, adopt it—I will not say to the total exclusion of the other, but certainly upon every possible occasion in preference; for there is one thing upon which much stress can be laid, which is, that the unsuccessful attempts to accomplish the new method, instead of increasing the difficulty of delivery by the older plan, tends rather to make the latter more easy; at any rate, the fœtus can easily be returned to its original position.

Prof. C. Esterle has shown great skill and patience in endeavouring to detect during the seventh and eighth months of pregnancy the position of the fœtus, and deserves great praise for practically showing the possibility of version at that time; but as such opportunities are rarely to be found in ordinary daily practice, and as it scarcely can be expected his skill and experience are to be met with except in such as devote their time specially to that subject,—and as, coupled with other reasons, it is a practical fact that the majority of the malpresentations and complications of labour do not come to the knowledge of the practitioner till labour has set in, or till, as in hemorrhage, active interference is necessary,—I think that I am not overrating the value of the above easily-applied and safer method of version; for the finger inside the cervix gives a sure indication as to progress of the pressure on the breech above.

It may also be noticed as an advantage that the membranes need never be ruptured, as was generally practised, though not always, by the older method.

The third point I proposed for consideration was the question—To what cases is the above method more peculiarly applicable, and to what conditions is it exclusively so?

From the foregoing remarks, it will be readily seen that it can be applied at the earliest period a malpresentation is detectable. As soon as the finger can enter the cervix, so soon can version be performed, converting all forms into breech presentations. In malpositions of the head, perhaps it may be found capable of improving its position without having recourse to complete podalic version; in puerperal convulsions, diminishing the great risk in such cases from the addition of the hand to the contents of the uterus; in narrow brims, when version is decided upon, it will save the pressure upon

the os uteri against the projecting parts of the brim. To these points I shall allude in a future communication, and shall for the present confine myself to the advantages this method gives us in *placenta prævia*—at least, in every form of partial insertion. I have as yet had no opportunity of testing its applicability in absolutely central insertion, and perhaps we can scarcely hope for any peculiar advantage in that condition, which becomes so formidable if the os continue undilated. Although in most writings it is said the hemorrhage which in these cases has already occurred is generally sufficient to dilate the os, still many will present such a prolonged rigidity of that part, that death may take place before entry of the whole hand can be effected; and it has so happened that this form has in my own experience been by far the more frequent. These cases are the more serious because of the difficulty of obtaining a firm compress on the bleeding part. The contracted cervix generally has been seen where the hemorrhage has begun sometime before the full term (at the seventh or eighth month), when the lower portion of the uterus has not fully expanded.

Case 1.—Mrs. B., of Walworth, a delicate, feeble woman, the mother of six children, about thirty-eight years old, and eight months advanced in pregnancy. For the four previous days she had been losing blood in gushes. Her attendant requesting me to see her, I found her, on my arrival, very blanched, pulse thready, very weak, and quick; voice very feeble; altogether in an exceedingly low state from flooding. The os uteri was about the size of a crown-piece, thickened, irregular, and unyielding; the placenta presenting three parts across it; the membranes not broken; no pains had appeared; fœtus alive; movements feeble and convulsive; head presenting. Blood was still oozing. I, therefore, gave her plentifully of spirits and water; and introducing my hand into the vagina, I endeavoured to gently dilate the os. After trying some time, with very little effect, I thought it would be a great advantage if I could turn without entering the uterus, and put in practice the method previously described. The head receded to the left side as the breech came down. The shoulders then presented; but in a minute more they followed the head, and then the leg was easily felt through the membranes. Having broken them, I drew the knee into the vagina, and, plugging the os with the leg and breech, and keeping up a slight traction, I waited to allow rallying further. I also gave a dose of secale, with stimulants, to ensure contraction subsequently. As the os expanded, I gently drew down the fœtus, which was fully born (but dead) in about three-quarters of an hour. She did not lose half an ounce of blood during the whole process, nor during the removal of the placenta, which was slightly attached, but not adherent. Had she lost another gush, she must have died at the time. However, the quantity already lost had been too much; for, after lingering out three weeks of extreme exhaustion, she died.

Case 2.—Mrs. ——, of Castle-street, Borough; aged about thirty-five; thin, but tolerably healthy; eight months advanced in her eighth pregnancy. During the fortnight previous to my seeing her, she had lost blood by gushes and clots, at no time very severe; and about six hours before there was a small gush, with clots, and at the same time the liquor amnii escaped, but no pains followed. I found there was still some bleeding, though not extensive, with clots in the vagina. Her appearance had not been affected by the loss. The os had been dilated to half the full size, but now it was so excessively irritable, that upon the slightest touch of the finger it contracted so that only one and at the most two fingers could enter. The placenta was stretched two-thirds across it, posteriorly. It here seemed to me that if the leg and breech of the fœtus could be made to form the plug, the patient would be secure. The only way possible, without much risk, was to endeavour to put in practice the method before described, which was easily done, though not quite so readily as in Case 1, in consequence of the irritability of the cervix; still it was effected, notwithstanding the escape of the waters, without any force or pain, and under circumstances which would have precluded entry of the whole hand. By means of two fingers in the os, I drew down the knee into vagina; and Mr. Woodman (the senior obstetric clerk) having passed a loop of tape through the bend of the knee, I left the case in his charge, with instructions not to hurry the birth, but to keep up gentle traction, and to plug the cervix. In about three hours, the os relaxed and dilated to its full size, when the fœtus was brought down. It had apparently been dead for a day or two. Not the slightest drop of blood was lost during the whole process of turning and extraction, and the woman recovered without a single bad symptom.

Case 3.—Mrs. P., a fine, healthy person, well made, who had always had rapid labours. At the time I saw her, she was at full term of her seventh pregnancy. Having been seized twice within the previous month with a gush of blood (each time about half a pint) she was kept quiet. Twenty-four hours before I saw her, she had had the third gush, which had continued ever since. I saw more than a pint, and the nurse said she had lost altogether as much more. At the time of my visit, the face was not much blanched, nor was the pulse much affected. The os uteri was fully dilated, the placenta half-way over it; uterus perfectly inactive. The liquor amnii had escaped, but the fœtal head was high up; no violent bleeding, but considerable oozing. There can be no doubt that the older method of version would have been very practicable. Still, as every circumstance was favourable, I turned by the method previously recommended, and found it very easily and speedily managed. In about two minutes sooner than I expected, the foot was in my hand, and drawing the leg through and the breech to the os uteri, I kept up a gentle traction. No uterine action ensuing, secale was given. In a quarter of an hour pains

appeared. After the breech had passed the cervix, finding the funis pulsating, I hastened the delivery, good pains assisting. The child, although not breathing when born, was soon recovered by artificial respiration. No hemorrhage appeared during the operation or expulsion of the fœtus. The placenta not being thrown off soon, and a little hemorrhage appearing. it was removed, being rather firmly attached.

Case 4.—Mrs. C., Borough, aged about twenty-eight, of strong constitution. At about three weeks to completion of full term of her fourth pregnancy. About three weeks before, she had been seized, after a fright, with sudden and severe flooding. This passed off till twelve hours before I was called to her, when she was taken with a sudden flooding, nearly equal to a chamber-vesselful, accompanied by faintings, which continued during the twelve hours before I saw her. I found her perfectly blanched: pulse very feeble; extremities cold; could still swallow. and had taken about half a pint of brandy. The os uteri was about the size of a five-shilling-piece; the placenta protruding, apparently centrally attached, but on carefully searching I found a portion of it thinner at the posterior aspect, and, carrying the finger backwards, the membranes were reached, unbroken; the head presenting. Being anxious to have command of the labour, with as little shock as possible to the patient in her very low condition, I employed the method mentioned above, with so much ease and rapidity that in about one minute the foot was in the vagina. In this case the membranes were ruptured at the beginning of the version; this, however, made no difference to the accomplishment of it. Continuing to make a plug of the breech, I waited till the system had rallied by stimulants, and in order to allow the os to expand and the pains to come on. In about an hour and a half the pulse improved, and two doses of secale were given; about half an hour later, gentle traction being employed, the child was born dead. The placenta was expelled by natural efforts. No hemorrhage occurred after I commenced version. She recovered satisfactorily.

Case 5.—Mrs. —, the mother of eight children, about eight months advanced in pregnancy, was taken in the night—seven hours before I saw her—with sudden flooding, which was estimated at more than two quarts. Four hours after she sent for her attendant, who found her with her extremities cold, blanched, and lying before the fire. From that time till I saw her she had been losing blood by continuous oozing, considered to be about half a pint. On my arrival, I found her blanched, pulse scarcely perceptible, voice strong, and considerable muscular power. The os was dilatable, about the size of a crown-piece, but had not been so more than an hour. The placenta came at that time only to the margin. It had before been felt over the os in part. The liquor amnii had escaped; no pains. As the oozing was still going on, I was anxious to secure a plug. I therefore directed

and assisted Mr. Wadkins (the senior obstetric clerk) in performing version by the method above described, which was done in a few minutes. The moment the head rose into the iliac fossa, the shoulder presented, and then as instantly the foot was in the hand of the operator. During this time we gave largely of stimulants, and apparently the patient had still considerable power. By the weight of the arm traction was kept up against the cervix, and in about half an hour the child was born (but dead), without any hemorrhage at all. The uterus contracted well, and the placenta came away naturally. Warmth and stimulants were employed; but the pulse began to flag still more, and the powers gradually failed, till, about an hour after, she died, rather suddenly. It seems to me that if the version had been performed as soon as the os was capable of admitting a finger, the draining of blood might possibly have been prevented. The quantity lost at first was not precisely known till after death, and, coupled with the muscular power and uterine pains, there did not seem any justification for retarding delivery, as has been recommended in extreme exhaustion.—*Lancet, July* 14 *and* 21, 1860, *pp.* 28, 55.

97.—ON THE OPERATION OF TURNING IN LABOUR OBSTRUCTED THROUGH COARCTATION OF THE PELVIC BRIM.

By Dr. Robert Barnes, F.R.C.P., Physician to the Royal Maternity Charity, &c.

On the 2nd of February, 1858, my assistance was required at a protracted labour by a midwife of the Royal Maternity Charity. The patient, a pluripara, had always had lingering labours, and the last child was still-born. She was now in labour at term. She had been seen by a surgeon at ten p.m. the day before. The head was at that time presenting at the brim, and it was reported that she had been in labour some hours without progress. I saw her at three a.m. on the 2nd. The pains had then remitted; the patient was weak and restless, her pulse small. The head was in the first position. The sacral promontory projected so as to be easily reached by the finger. The antero-posterior diameter was estimated at 3·50." The cranial bones overlapped considerably at the sagittal and lambdoidal sutures, the skull being somewhat flattened between promontory and pubes. But still the full transverse diameter of the head had not entered the strait, nor could it do so unless under the influence of considerable additional force. A fair degree of uterine contraction now returning, I waited an hour; when, finding that the force required could not be supplied by the patient, that there was no advance, that symptoms of exhaustion were appearing, and being satisfied that the forceps would not deliver a living child, I lessened the head and delivered by crotchet. The extraction was easily accomplished; for the moment the

capacity of the head was diminished, strong expulsive action set in. The placenta was removed in five minutes, and the uterus contracted well. The patient recovered favourably.

Second Labour.—The woman had disregarded my injunction to have labour brought on prematurely in the event of another pregnancy, so that on Feb. 15th, 1859, she being in labour at term, my assistance was again required. When I saw her, the os was fully expanded, head and foot presenting. As the patient's strength was still good, I determined to deliver by turning. The version was effected under chloroform without difficulty, the presenting leg being brought down. The head was jammed for twenty minutes in the brim between the promontory of the sacrum and the symphysis: it was dragged through under considerable force, emerging into the cavity with a sudden slip. The child was born asphyxiated, but in half an hour cried loudly. It was not of a large size; the head was much compressed at the sides. Mother and child did well.

Commentary.—This case gave me both satisfaction and surprise. I little expected that a living child would be born after the head had been subjected to such great and protracted compression. Had not the head been of somewhat less than average dimensions, the result would not, I believe, have been so favourable. Still in this case, all the conditions of safety to the mother and child could be secured by bringing on labour at the end of the eighth month, and resorting to version should the head not freely enter the pelvic brim. Here were a conjugate diameter of 3·50″, and a head below the average dimensions. In most instances, the natural forces, if of ordinary vigour, might overcome a disproportion of this kind. In this particular case the natural forces were insufficient, and it became necessary to choose between the forceps, turning, and craniotomy. I postponed craniotomy as the last resource. It is possible that the forceps might have been successful; but cases which I shall hereafter relate will show that, except under special circumstances, this instrument will not effect delivery obstructed through disproportion, with less risk to the mother or with more safety to the child, than can be attained by turning. The last operation could place the child's head in the most favourable position for entering the contracted brim. The hold obtained upon the legs and trunk would give the obstetrician a degree of power which neither the natural forces, nor the forceps, nor both combined, could supply. The power so obtained, when exerted, would equalize the transverse diameter of the child's head and the conjugate diameter of the pelvis most directly, and with the least possible violence. Brought into the brim base foremost, the plastic skull most easily adapts itself to the rigid distorted contour. The compressing force, first bearing upon the skull in its extreme transverse diameter, soon lessens this, because above the promontory and on either side there is no obstacle to the expansion of the skull. On the other hand, the forceps cannot exert the requisite degree of compressing force

under equally favourable conditions. The compression cannot be applied directly to the transverse diameter of the skull; it must fall either obliquely or in the occipito-frontal diameter. In either case, the compression, by lessening the occipito-frontal diameter, must increase the bulging of the bi-parietal diameter, and bring this—the obstructing part—into more violent impaction between the promontory and symphysis. The instrument thus adds to the very difficulty which it is sought to overcome, and calls for a supererogatory increment of force to balance that excess of resistance which it has itself created.

Turning has this great advantage, that it brings no artificial difficulty to enhance that difficulty which already exists.

The case before us does not mark the limit of the applicability of turning in contracted pelvis. It is true that with a conjugate diameter of 3·50″ and a small head, the passage through the pelvis was not effected without some difficulty, and some risk to the child's life. But experience has proved that a living child of full size may be extracted through a yet smaller pelvis with safety to the mother. We must, therefore, pursue the inquiry, and study fresh cases, before we can determine the limits of the conservative application of turning *in* contracted pelvis.—*Lancet, April* 21, 1860, *p.* 390.

98.—*Pregnancy with Duplex Uterus.* [In a work lately published by Professor KUSSMAUL, of Heidelberg, a most elaborate array of cases is given, illustrative of pregnancy in the perfectly developed duplex uterus. Amongst others occurs the following anecdote.]

"One of our most celebrated accoucheurs met with a remarkable case in a woman who had a duplex uterus and a double vagina. She asked him whether she might become married; and he gave her the advice, that if she did so, she should only make use of one of her vaginæ. In course of time she was brought to bed of a child. *Sed quid fit?* After three months she again fell in labour, showing clearly that she had twice conceived, at different times." "Truly a most remarkable case!" adds old Baldinger. Lobstein states, that he "delivered a woman of two infants, one a month after the other; and was able to convince himself that she had two uteri, and to each a distinct vagina."

In cases of parturition with such duplex uteri, labour may occasionally be quite natural; but generally it is the contrary. The malformation of the womb and of the vagina, as well as the abnormal shape, size, and axis of the uterus, may be assigned as causes which render parturition difficult and perilous. Many of the women who have borne children under such circumstances have died during the puerperal stage.—*Edin. Med. Journal, July*, 1860, *p.* 61.

99.—PUERPERAL CONVULSIONS SUCCESSFULLY TREATED BY SUBCUTANEOUS INJECTIONS OF MORPHIA.

By Professor SCANZONI. of Wurtzburg.

Since the attention of the medical profession was first directed by Dr. Wood, of Edinburgh, and more lately by Hunter and Béhier, to the advantageous effects of subcutaneous injection, especially of narcotics, Professor Scanzoni has employed this method with success in numerous cases of neuralgia, hyperæsthesia, &c.; but he attaches especial importance to the following case of puerperal convulsions, because it seems to prove, in accordance with the views laid down by Hunter, that the subcutaneous application of narcotic agents furnishes a means of acting on abnormal irritations of the brain with greater rapidity and certainty than the administration of the same remedies by the mouth. It will, doubtless, be admitted that opium, and its different preparations, deserve the first place in the treatment of puerperal eclampsia. In his own experience, the observation of a large number of cases has convinced Professor Scanzoni that a kind of intoxication produced by opium leads with more certainty to a favourable termination than any other means recommended in this terrible disease. But, unfortunately, it is not always possible to administer a sufficient quantity of opium or morphia; sometimes the comatose condition of the patient, at other times the rapid succession of paroxysms, prevents administration by the mouth; and opiate enemata are occasionally rejected as soon as they are received. The subcutaneous injection, however, supplies the means by which these difficulties may be overcome, and a sufficient quantity of opium introduced into the system to render its effects certain. Numerous experiments have convinced the author that, although the effect of this method is not always persistent (the neuralgiæ, for example, are not always cured by it), yet there are constantly produced,—within a very short time, often a few minutes, after the injection,—certain phenomena, which can leave no doubt as to the action of the opium upon the brain. Such symptoms are drowsiness, giddiness, headache, sickness, feeling of constriction in the throat, even vomiting, and depression; or, if the dose is large, somnolence. These facts, taken along with the known effects of the subcutaneous application in delirium tremens, mania, chorea, tetanus, &c., induced him to try the same treatment in puerperal convulsions, and with the most satisfactory results. After three injections of meconate of morphia there occurred only two attacks in nine hours, while previously there had been three attacks in an hour and three-quarters. This diminution of the convulsions after the injections is so much the more remarkable, since experience has shown that, as a general rule, the paroxysms become not only more violent, but follow at shorter intervals as the labour advances. And although the author does not imagine that he has discovered in the subcutaneous injection an infallible panacea for

this dreadful malady, he is of opinion that the following case should induce physicians to give this means a trial :—

Case.—D., aged 21, primipara, strong and robust, was brought into the lying-in ward at a quarter to eight o'clock on the morning of June 8th, 1859. Labour had commenced in the night, and she had been seized with nervous paroxysms and loss of consciousness; no account was given of the nature of the attacks; the patient remembered nothing of what had occurred during the night. The whole body, and especially the lower extremities, were œdematous; on the right side the tongue showed marks of being bitten by the teeth; the uterus corresponded to the pit of the stomach, and seemed sufficiently consistent; sounds of the fœtal heart distinct. On examination, the os uteri was dilated to the size of a sixpence, the bag of waters was partly formed, and the head presented; the urine was very albuminous, and exhibited under the microscope numerous fibrinous cylinders. At eight o'clock she was seized with a second convulsive attack, which was of a very marked character, and lasted for some minutes. On recovering consciousness, she could answer questions, although slowly. A third attack succeeded at a quarter to nine, a fourth at a quarter to ten, a fifth at a quarter to twelve, and a sixth at five o'clock,—the last the most violent. After the fourth paroxysm consciousness did not return, and the breathing became stertorous. At ten o'clock she was bled to about eight ounces, an emena with twenty five drops of laudanum was given, the body was put into a warm bath, while cold irrigation was applied to the head. As opium could not be administered internally, a solution of the meconate of morphia was now, at three different times, injected under the skin, the quantity amounting in all to about 10 grains (75 centigrammes) of opium. The labour advanced very slowly. At three o'clock next morning the membranes burst; the os dilated to the size of a half-crown; the head still high up above the brim; sounds of the heart very distinct. After this period the dilatation went on more quickly; at seven o'clock the os was larger than a crown piece, very extensible and dilatable, the head high up and immovable; complete loss of consciousness, profound coma. In these circumstances, which left little hope of saving the patient, and in spite of the high position of the head and the incomplete dilatation of the os uteri, it was decided to employ the forceps. Their application was by no means easy, but the extraction presented no difficulty. After a few tractions, a fœtus was born, which breathed feebly at first, but soon began to moan vigorously; the placenta followed. During the operation there was no paroxysm. Some wine and ten drops of tincture of amber and musc were now given to the patient, which revived her a little, but did not restore consciousness. At eleven o'clock, a seventh attack came on, but was slight and short; after which she became excited, and tried to escape, but towards morning she grew calm. At nine in the morning she could answer questions put with a loud voice. During the whole day she remained like

a drunk person; pulse 128. The musc was stopped; nothing but lemonade given. Towards evening the abdomen was somewhat painful. During the night there were several slight attacks of mania; she constantly attempted to escape. In the morning she answered rationally; pulse 108. The œdema had diminished, the abdomen was still tender; there was difficulty of breathing; and numerons râles, fine and coarse, in the lungs. Warm bath, lemonade, expectorants, were prescribed. In the evening the patient was completely herself again; pulse 132. June 11th and 12th: She slept well during the night, the expectoration becoming easy, and the pain of the abdomen relieved by fomentations and poultices; pulse 120; the urine contained little albumen, and no fibrinous cylinders. June 13th: Good condition; œdema gone, abdomen soft; some incontinence of urine during the night was relieved by leaving in a catheter. All medicines were now suspended; the patient was put on good diet; and ordered to take every morning a glass of chalybeate mineral water. On the 17th there was no albumen found in the urine; and on the 21st the patient left the hospital with her child, being advised to continue the use of steel for a considerable time.—*Bull Gén. de Thérap.—Edin. Med. Journal, May*, 1860, *p.* 1045.

100.—*Post-partum Hemorrhage.* By P. Hubbert, Esq., Croydon.—Much has been written on kneading, plugging, emetics, ice, &c., none of which I think can be fully relied upon, but I have always succeeded by the simple plan of injecting about half a pint of very cold water (iced if you can get it) into the rectum, giving at the same time cold brandy-and-water, with a drachm of Curtis's liq. secalis cornuti in it. There is less difficulty in this operation than in injecting the uterus, which, I imagine, when attempted, is not always done. The application of the infant to the breast frequently fails to produce the desired contraction. I invariably carry in my midwifery case a small elastic enema instrument.—*Lancet, Sept.* 8, 1860, *p.* 250.

101.—ON THE DIAGNOSIS OF DROPSY OF THE FALLOPIAN TUBE.

By Dr. J. Y. Simpson, F.R.S.E., Professor of Medicine and Midwifery in the University of Edinburgh.

[The characters by which it can be determined whether a cystic tumour, lying in the pelvis, is due to the dilatation of a Fallopian tube, are shortly these:]

First. *Its free and independent Mobility.*—The tumour is freely moveable in the cavity of the pelvis, and does not move synchronously with the uterus; but, on the contrary, it remains at rest when the womb is moved, and can be readily moved about by the exploring

finger when the womb is kept firmly fixed, either by the hand placed above the pubes, or better still, by means of the uterine sound introduced into its cavity.

Secondly. *Its elongated Form.*—As the fluid collects in and distends the tube in nearly all its length, the resulting tumour is of an elongated conical form, with the rounded base corresponding to the obliterated fimbriated extremity and the apex at the upper angle of the uterus.

Thirdly. *Its wavy Outline.*—In all cases the surface of the tumour feels more or less undulating and indented, for, in most instances, the distended tube is folded once or oftener on itself, and tortuous; and when it is not thus wavy and convoluted, the smooth outline is interrupted by the pressure of bands of plastic fibrine stretching over and indenting it.

I have also referred to the comparative size of the tumour and its situation in the ordinary position of the Fallopian tube as guides—though certainly of less value—in enabling you to discover and recognise the disease. If you have satisfied yourselves in any case as to the existence of the characteristics which I have attempted to pourtray, you may proceed at once to confirm your diagnosis of the disease, and initiate your treatment by introducing an exploring needle into the interior of the cyst, and examining the fluid that escapes, which, as I have stated, is usually perfectly limpid and clear. It is, no doubt, a matter of considerable difficulty to make out clearly in certain cases the actual presence of a tumour with all the characters I have described; and to decide positively that it is a dropsy of the Fallopian tube will demand a certain amount of experience on your part, and no small care and caution in conducting the examination. But the difficulties of the case will be lessened, and the diagnosis will be rendered more certain, if you will always take the precaution to investigate the pelvic organs by means of both hands simultaneously—one being placed over the fundus uteri, and pressing down upon the pelvic organs through the abdominal walls, while two fingers of the other hand are passed into the two canals at the outlet of the pelvis, for the purpose of examining the state of its contents from below. Where the patient has previously had a family, the abdominal walls are soft and flabby, so that the pelvic organs can be readily felt through them; and where they are firmer and less yielding, by putting the patient under the influence of chloroform they will be so far relaxed as to enable you to have all the parts thoroughly at command.

Prognosis.—In the great majority of cases the disease has remained unrecognised and unsuspected during the lifetime of the patient, and has only been discovered after death when the body was subjected to a post-mortem examination; and this is more particularly the case where the cyst is not of very large size, and its contents consist simply of a serous fluid. But where some morbid process has been going on in the interior of thecy st, and the altered mucous membrane has

become the seat of inflammation in its higher grades, or where it has been subjected to injury, so that the contents become mixed up with purulent effusions and bloody extravasations, the ultimate issue is apt to be less favourable. For in such cases it sometimes happens that the cyst-wall becomes ulcerated through or bursts, and the morbid and irritating fluid escaping into the cavity of the peritoneum lights up a degree of inflammation on its serous surface that proves rapidly fatal. In some few cases, again, the disease may be recognised during life, even when the cyst is of the simplest kind, but where it is of sufficient size to give rise to the series of symptoms to which I have alluded. In such cases you may succeed in diagnosing the disease in the manner I have described to you, and in conducting it to a favourable termination by an appropriate plan of

Treatment.—Medical treatment of any kind seems to be of but little avail for the cure of this kind of malady. The only effectual means of affording relief to the patient, and her only hope of a permanent cure, is the removal of the fluid by puncturing the cyst.—*Med. Times and Gazette, July* 21, 1860, *p.* 49.

102.—CASE OF SPONTANEOUS EXPULSION OF A FIBROUS TUMOUR OF THE UTERUS.

By Dr. Joseph Griffiths Swayne, Physician-Accoucheur to the Bristol General Hospital.

The case I am about to relate is a good instance of the manner in which the *vis medicatrix naturæ* may sometimes, under favourable circumstances, effect the removal of a tumour which is quite beyond the reach of a surgical operation.

A lady, aged 37, previously in good health, married on August 14th, 1856, and on July 22nd, 1857, miscarried, when about eight weeks pregnant. At that time she was staying in Edinburgh, and was under the care of Dr. Duncan. About six weeks after the miscarriage, she experienced considerable pain in the left iliac region, accompanied with rigors and feverishness; and, on placing her hand upon the part, thought that she could detect a slight swelling. About ten days after this, she was attacked in the night with a great deal of hemorrhage from the vagina, and afterwards with an increase of pain in the part affected. The pain was more severe at some times than at others, and never entirely left her. About six weeks after the hemorrhage, she again went to Edinburgh, and consulted Dr. Duncan, who examined her, and detected an enlargement on the left side of the uterus. Soon afterwards she left Edinburgh and went to Kissingen. She now never entirely lost the pain, and always found it increase at the menstrual periods. At this time she could herself feel the tumour, especially in the morning. She returned to England in July 1858. She was then in tolerable health in other respects, but yet was never

entirely without pain, and had a return of hemorrhage at each menstrual period. In October 1858, she came to Clifton, and soon after consulted Dr. W. Budd, who then examined her, and distinctly felt the uterine tumour. He prescribed medicines for her which had the effect of restraining the hemorrhage at each period, until, on February 24th, 1859, she menstruated for the last time, and then became pregnant.

She continued pretty well during her pregnancy, with the exception of a good deal of sickness, and occasional pains in the left side, especially in walking. She was not able to distinguish the tumour after the second month of pregnancy. On November 26th, 1859, I was sent for to attend her in her confinement. The first stage of the labour went on favourably, although rather slowly, and occupied altogether about forty-eight hours. The first part of the second stage was tolerably expeditious; but, towards the end of this stage, the progress of the head became less rapid, although the pains still continued powerful. At last the head became completely arrested at the lower outlet, and remained in that position for four hours. As the pains began to diminish very much in force, I applied the short forceps, and delivered her of a fine male infant. The placenta was expelled without any hemorrhage. For the first week after delivery, both mother and child went on well. When I called on the eighth day (Sunday December 3rd), I found my patient feverish, and complaining of headache, which she attributed to taking rather too much animal food on the previous day. I prescribed an aperient and a less stimulating diet, and called to see her on the following Tuesday. She was still rather feverish; the pulse was 104; and she complained of dull aching pain, with slight tenderness on the left side of the abdomen, just above the ilium. I ordered her eight grains of Dover's powder, and three grains of hydrargyrum cum cretâ, to be taken at bedtime; together with poppy-head fomentations to the abdomen. On the following Thursday, the symptoms were much the same; and she was directed to repeat the powder, and apply bran poultices. As the pain and tenderness still continued, I agreed to meet Dr. Budd in consultation on Tuesday, December 12th.

We found her in much the same condition as regards the general symptoms. On examining the abdomen, we felt that the fundus uteri reached nearly as high as the umbilicus; and the uterus itself was as large and hard as an uterus just after delivery, before the expulsion of the placenta. The enlargement was more perceptible on the left side of the uterus, which was also somewhat tender on pressure. We were both of opinion that some unfavourable change, such as subacute inflammation and softening, was going on in and around the uterine tumour, which had been ascertained to exist before the occurrence of pregnancy. She was ordered to repeat the Dover's powder and fomentations, and also to use an opiate suppository. On the next day, the febrile symptoms and tenderness were somewhat

increased. When I called on Friday, December 15th, the nurse showed me two pieces of a fleshy substance, which had passed *per vaginam* since my last visit; the largest of these pieces was of the size of a hen's egg, and appeared to be made up of concentric layers of fibres. On examining it microscopically, I found that it consisted of unstriped fibres resembling those of the uterus. The expulsion of this substance was attended with a sanguineous discharge, like the lochia in appearance, and at first not at all offensive. This continued for several days, during which time a great number of smaller pieces were expelled, sometimes with distinct pains; the discharge and febrile symptoms still continuing. To promote their expulsion, ergot was given on each day, together with opiate enemata and suppositories, to relieve pain and tenderness. On Monday, December 18th, we found that the portions expelled were assuming a darker colour and a putrid appearance; and the discharge was becoming of a reddish-brown hue, and very offensive. This was accompanied with a short hacking cough and hectic fever, and the pulse still kept up to 100 or more; although in other respects the general condition of the patient was not unfavourable. She was put upon a generous diet, and ordered to take quinine; and the nurse was directed to syringe out the vagina frequently with chlorinated lotions. During this fortnight, the offensive discharge still remained, and broken down shreds and portions of tumour were repeatedly coming away. The uterus, which at first could be felt nearly as high as the umbilicus, gradually diminished in bulk and subsided in the abdomen, until, on Monday, December 25th, we could scarcely feel the fundus above the pubes. The whole of the tumour appeared by this time to have been expelled. On collecting all the portions and putting them together, they were found to form a mass of about the size of a man's fist, and to weigh rather more than half a pound. The treatment adopted on December 18th was continued up to this time. The discharge now began to improve very sensibly, both in quality and quantity, and the hectic fever to diminish; while at the same time our patient was steadily gaining strength.

On Tuesday, January 10th, 1860, there was a very slight pale mucous discharge, which was not at all offensive. The uterus was now so much reduced in bulk that it could not be felt. There was no fever; the pulse was 84; and the cough had almost entirely ceased. From that period she progressed rapidly, and in another fortnight had completely recovered.

Remarks.—In this case, before the occurrence of pregnancy, the presence of a fibrous tumour occasioned considerable hemorrhage at each menstrual period, and also a certain amount of pain, symptoms which are commonly noticed in connection with these morbid growths. But after the patient became pregnant, it is surprising how little the process of gestation was interfered with. There was a marked improvement of health, and no symptoms of the presence of a tumour

was observed, except occasional pain in the left side after walking. During the progress of parturition the tumour gave rise to no untoward complication, except, perhaps, that it had the effect of retarding the second stage of labour; but of this I am by no means certain. After the birth of the child, a fibrous tumour is very likely to occasion *post partum* hemorrhage, from its interfering with the due contraction of the uterine fibres; but in this case nothing of the kind took place. But it is during the changes which take place in the uterus after delivery, that we have most reason to dread the results of a fibrous tumour. As Dr. West remarks, "it may interfere with the process of involution of the womb after delivery, and may either itself undergo a morbid softening and disintegration, or may be the occasion, either in connection with inflammation of its own substance and of that of the womb, or independently of them, of peritonitis, always dangerous, too often fatal." In the case I have just described, the termination was much more fortunate. The tumour no doubt was one of those which are imbedded in the walls, and between the fibres, of the uterus, with which they have a very slight connection. In all probability, it was much nearer the mucous than the serous surface of the womb, and thus it was that during the degeneration and absorption of the uterine fibres after delivery, it lost its connections with the uterus, and consequently its vitality, and this gave rise to inflammation and ulceration of the uterine fibres between itself and the mucous surface, until a sufficient opening having been made, it was cast off piecemeal into the cavity of the uterus; and thus the natural processes in this instance, instead of being destructive, as they usually are, were curative, and were attended with the happiest results.—*Brit. Med. Journal, May* 12, 1860, *p.* 358.

103.—ON THE DIAGNOSIS OF OVARIAN DROPSY.

By M. Boinet.

An elaborate paper on this subject, from the pen of M. Boinet, has been published in the 'Gazette Hebdomadaire de Médecine et de Chirurgie' for January 6th, February 3rd and 17th, and March 9th of the present year. As the essay represents the opinions of a practitioner who is well known to have had large experience in the management of the disease in question. we subjoin a copious abstract; abridging little but the histories of some of the cases which he relates in support of his views.

The affections which are most commonly mistaken for ovarian cysts, are—ascites, pregnancy, extra-uterine pregnancy, tympanitis, encysted dropsy of the peritoneum, fibrous and cancerous tumours, masses of indurated fæces, abscesses in the iliac fossæ, retro-uterine hæmatocele, hydatid cysts, &c.; and yet all these have only the appearance of varian cysts, and may be recognised by particular signs. But it is

not enough merely to diagnose an ovarian cyst from any of the other abdominal tumours; it remains to be ascertained whether it is unilocular or multilocular, simple or compound; whether it is complicated with ascites, pregnancy, fibrous tumours, &c.; whether the contained fluid is serous and clear, or coloured, thick, thready, gelatinous, sero-purulent, &c.; whether the cysts are moveable or adherent. In its bearing on prognosis and treatment, the recognition of an ovarian tumour in any of the varied states in which it may present itself, is of the highest importance.

The characteristic signs of ovarian cysts differ, according to the nature of the cyst and its contents, and according to the presence or absence of complications. One of the first symptoms is a dull pain, a feeling of weight, a dragging, in one of the iliac regions. This feeling often arises from turgescence of the ovary, and is especially felt before or during the catamenial periods: at other times, the pain is only excited by pressure or by certain movements. These early symptoms, it is true, have no great diagnostic value at first; but at a later period, when the abdomen has become enlarged, and it has become impossible to discover by palpation on which side the cyst has arisen, then these symptoms become important, as indicating its point of origin.

Afterwards, when the disease has made some progress, there appears in one of the iliac regions a more or less painless tumour, readily felt by the hand, lobulated or round, and offering an equal elastic resistance. The tumour often passes the pubes and reaches the umbilicus before the patient has suspected its development; and then the medical man, called for the first time, ascertains with difficulty on which side it has commenced. At the end of a certain time the abdomen acquires an uniform development and the size of an advanced pregnancy. If the patient is young and in the way of becoming pregnant, she is likely to deceive herself willingly; and it is only when the duration of the enlargement has exceeded the natural limits of gestation, when the abdominal tension increases, when a sensation of pain is felt, and when functional disturbance of the abdominal and thoracic viscera appear, that she recognises that she has a dropsy in spite of her generally good health.

There are special signs which characterise ovarian cysts at different periods of development. When the surgeon is called in at the commencement of the disease, particular attention should be given to the form of the abdomen. It will be often recognised, on careful examination, that it projects more on one side than on the other. If it appears uniform, palpation will in certain cases discover the presence of a tumour, the limits and origin of which may be ascertained. In young girls and in women who have never borne children, it is sometimes difficult to determine on which side the tumour has commenced; since, while it has been growing, the mass of the intestines has been pushed over to the other side, so that the abdomen is equally

distended: in such cases, careful percussion over every part of the abdomen discovers a tumour on one side and the intestines on the other. And, when the abdominal walls possess a certain degree of laxity as in women who have borne children, and the abdomen is not very voluminous, the tumour will change its position with the side on which the patient lies, and, in doing so, often causes a kind of undulation to be felt.

An ovarian tumour having been ascertained to exist, is it liquid or solid? Circumscribed pressure on the tumour, carried along the iliac fossa, will cause a more or less evident fluctuation to be recognised, and will cause an elastic depression to be perceived, which prevents the disease from being confounded with a fibrous tumour, impacted fæces, or any other solid swelling. Percussion gives a dull sound along the whole extent of the tumour; and fluctuation is often felt at the same time. Examination by the vagina or rectum proves nothing, so long as the tumour remains in the ilio-lumbar region, and is not yet developed in the pelvis; but when it has acquired a certain development, vaginal examination may be useful in ascertaining the side on which the tumour has originated, if this can be ascertained in no other way; for the tumour, in growing, carries the uterus with it, and gives this organ an oblique direction, so that the neck is turned to the opposite side from the fundus. This sign, important as it is, should not be had recourse to unless it is otherwise impossible to discover from which side the tumour has grown, and unless such discovery is of absolute importance.

At a still more advanced period, when the abdomen is uniformly enlarged, pressure and percussion must afford the diagnosis. By pressure are ascertained fluctuation, inequalities, and points of hardness. If the abdominal walls are not too tense or œdematous, it is possible, by pressing in the abdomen at every point, to feel masses varying in extent, mobility, shape, and size, formed of more or less resisting tissue, sometimes hard, sometimes soft and elastic. These masses, which resemble spherical bodies somewhat flattened, are sometimes so large, so divergely placed, that they simulate displacement of the liver, spleen, or kidney. Sometimes the sense of fluctuation is only partial and indistinct, and the different parts of the tumour offer an unequal resistance. In these cases, there is either a multilocular cyst, or a cyst or tumour complicated with ascites. In other cases, fluctuation is general, and more evident than even in the highest degree of ascites.

Percussion is the principal and most certains means of recognising (which may be done in by far the greater number of cases) whether a cyst is unilocular or multilocular, multiple or complicated; whether the liquid it contains is serous, thick, thready, or gelatinous; whether the tumour has several lobes, and what is their size, and to what extent they correspond with the abdominal walls. The progress of the disease, the more or less severe pains which the patient has experi-

enced, the more or less evident signs of inflammation which have appeared on the abdomen, the state of the general health, will also aid in pointing out the nature of the fluid, whether purulent or seropurulent, &c.; and palpation will discover the presence of tumours, of greater or less hardness, which may complicate the cysts.

After these general considerations on the diagnosis of ovarian dropsy, the special characters of each variety of cyst remain to be described.

Simple cysts, when they have occupied the whole abdomen, and the distension is uniform, present the following symptoms:—

The abdomen is projecting, so that one would say that the patient had arrived at the last stage of pregnancy. If, on percussing gently with one or several fingers, or, still better, on giving a slight fillip on any one point of the abdomen, the impulse of the fluid is plainly felt by the other hand, the cyst is unilocular if, in addition, percussion gives a dull sound in all the anterior and inferior parts of the cyst, and resonance only at the epigastrium and in the flanks on lower and lateral parts of the abdomen, whatever be the position of the patient. This occurs because the intestines, pushed aside by the tumour, project only beyond the edges of the cyst. Whether the patient is caused to lie either on the right or on the left side, the dulness is never displaced, but is always found at the same points and in the most elevated part of the cyst. These signs are constant, and enable an unilocular ovarian cyst to be distinguished from ascites, for instance. Another circumstance that may aid in the diagnosis is the generally good state of the health, and the rarity of swelling or œdema of the lower limbs.

In one case only of unilocular cyst has M. Boinet observed these diagnostic signs to fail. The case was one, which he observed with M. Putel, of a woman aged 60, in whom percussion denoted the presence of a serous fluid, and fluctuation was so distinct that the case might have been supposed to be one of ascites, if other signs had not denoted the presence of ovarian dropsy. A trocar was introduced; but no fluid escaped. On introducing a thread of iron wire through the canula, a thick gelatinous mass was drawn out. Nothing in this case led to a suspicion of the nature of the tumour, for percussion constantly gave the same results; the only circumstance which could have, under other conditions, thrown any doubt on the diagnosis, was that the patient was much emaciated, had a dry yellowish skin, and showed traces of the cancerous cachexia; all the functions, besides, were tolerably well performed, except that the breathing was impeded. Examples of this variety of cyst are very rare.

When the abdomen has acquired a large size, it is sometimes difficult to ascertain on which side the cyst has arisen. In this case, recourse must be had to the history of the previous symptoms, and, as a last resource, to a vaginal examination.

Although the diagnosis of unilocular ovarian cysts appears easy, errors of diagnosis are often committed. A patient came to M. Boinet, telling him that she had an ovarian tumour, of the nature of which she had already been informed by two hospital physicians. M. Boinet, relying on their diagnosis, drew off a large quantity of serous fluid, and proceeded to inject iodine, according to the plan which he strongly advocates; but the patient at once uttered so loud a cry, that it was evident that the injection had been made into the peritoneal cavity. Purulent peritonitis set in; the pus was evacuated, and iodine was again injected several times; the patient recovered, and was in good health in 1859, six years after the operation. M. Boinet observes, that the severe pain at the moment of injection is a certain sign that the peritoneum has been injected instead of an ovarian cyst.

M. Boinet is aware of numerous cases of error of diagnosis between ascites and ovarian dropsy. For instance, a young girl, on whom he operated with MM. Danyau and Fontés, had already been treated for ascites in the Hôtel Dieu. Another case occurred in the person of a Norman lady, who had been tapped four or five times for ascites. M. Boinet threw in iodine injections, with the result of obtaining a radical cure.

Proceeding to speak of the diagnosis of the forms of ovarian cyst, M. Boinet relates the case of a lady aged twenty-seven, in whom M. Gendrin diagnosed the presence of an agglomeration of cysts, and advised merely palliative treatment; M. Boinet believed that there was an unilocular cyst of the left ovary, containing a thickish fluid, slightly thready, and probably of a chocolate colour; M. Velpeau diagnosed a cyst with thick walls, probably single; and M. Trousseau was of opinion that it was a simple unilocular cyst. It was finally determined to inject iodine, which was done by MM. Boinet and Bougoin. The cyst was unilocular, with very thin walls; it contained five *litres* of slightly thready chocolate-coloured fluid. The injection was repeated three times; and the patient recovered.

When the impulse of the fluid is not felt at all the points of a cyst, wherever percussion may be made, it may be said with certainty that the cyst is multilocular, or that the fluid which it contains is very thick. In these cases, the fluctuation is limited to the points in which the surface of each lobe corresponds with the abdominal walls. By a careful examination, it may thus be possible to count the lobes of which the cyst is composed—at least, the superficial ones; there is almost always a principal lobe, a mother-cyst, round which all the others are grouped. Occasionally the cysts are divided into several pouches by more or less apparent projections or depressions, which may sometimes be felt through the abdominal walls. The presence of tumours in the walls of the cyst, excessive thickness of the walls, division into several cells, and especially density of the contained fluid, all obscure undulation. Extreme distension of the cysts with a thick

liquid gives a remarkable sensation of vibration. In these cases, percussion is not enough; careful palpation in all directions must be employed. In cases of multilocular cysts, the general health is ordinarily not very good.

It remains to determine the nature of the fluid contained in the cyst, and its density. When it is serous and limpid, so as to be capable of flowing readily through the canula like water, fluctuation is free, easy, perceived at all points, resembling that of a considerable ascites. If the liquid is dense, thick, thready, glutinous, oily, fluctuation, although perceptible, is much less free. In cases where the liquid is too thick to give rise to fluctuation in the ordinary way, it may be felt by applying the two hands on the abdomen, and applying a to-and-fro motion, which will enable the fact to be discovered, that the liquid possesses a certain consistence.

If the patients experience or have experienced continued pains in the abdomen, if signs of inflammation have been present, if the abdomen has been or is slightly tender on pressure, the fluid, whether the cyst be single or multilocular, will probably be more or less coloured—reddish, sero-sanguinolent, coffee or chocolate coloured, &c. Multilocular cysts especially where the fluid is thick, glutinous, and unhealthy, present these varieties of colour. It may with equal probability be announced that the fluid is purulent when the patients have a continued slow fever, loss of appetite, vomiting, diarrhœa, emaciation, pains in the abdomen and especially in the cyst—in a word, the symptoms of hectic fever, or those which indicate the presence of purulent deposits.

To avoid confounding ascites with encysted dropsy—the mistake which is most commonly made—it is sufficient to remember that the course of ascites is generally more rapid, that the enlargement of the abdomen is more uniform; and that, in ovarian dropsy, the abdomen is more protuberant, and preserves the same rotundity on whatever side the patient may lie. But percussion affords the surest means of diagnosis: in ovarian dropsy, it gives dulness in all the anterior, superior, and inferior parts of the abdomen, and resonance in the flanks; while, if the abdomen is resonant above and dull at the sides, there is ascites; the intestines floating to the surface of the fluid, while in ovarian dropsy they are pushed aside by the cyst. In ascites, if the patient is caused to lie on either side, the fluid changes its position, and resonance can always be detected on the side opposite to that on which the patient lies; in ovarian dropsy, on whatever side the patient may lie, the dulness is always at the same point. Another sign which is rare in ovarian cysts, and almost constant in ascites, especially when the disease is of a certain duration, is the infiltration of the lower limbs, and the pressure of organic disease of the liver, heart, and other viscera.

In encysted dropsy of the peritoneum, fluctuation is much more difficult to perceive. It is circumscribed and limited, and is never

found except at the same points. The intestines lie below, and do not, as in ascites, occupy the most prominent part of the abdomen. In this form of disease (which M. Boinet has seen twice in males and once in a female), the liquid is either collected in a single cyst extending, it may be, from the xiphoid cartilage to the pubes, and from one hypochondrium to the other, or in several separate cysts placed very superficially. The phenomena of distension and fluctuation are manifested here as in ascites; but encysted dropsy always arises from peritonitis, is slow in its progress, forms a more projecting swelling than ascites, and produces less impediment in respiration and displacement of the intestines.—*British Med. Journal, April* 14, 1860, *p.* 288.

104.—FOUR CASES OF OVARIOTOMY.

By T. Spencer Wells, Esq., F.R.C.S., Surgeon to the Samaritan Hospital.

In the last number of the 'American Journal of the Medical Sciences' (July, 1860), Dr. Sawyer, of San Francisco, says that of seven cases of abdominal section made in California, on account of ovarian disease, six terminated fatally; and he asks if surgeons "always furnish a report of their unsuccessful cases as well as of their successful?" He concludes his paper with the remark, that he is "satisfied that the statistics of ovariotomy are entirely unreliable, because but a feeble fraction of the fatal cases are given to the public; while there is not a single successful operation that does not find its way, either directly or indirectly, into some of the medical periodicals of the day." Somewhat similiar opinions have been hinted, if not openly expressed, on this side the Atlantic. I am anxious, therefore, to assure the reader that he may find in the carefully-elaborated table appended by Mr. Clay to his translation of 'Kiwisch on Diseases of the Ovaries,' particulars of *all* the cases, successful or fatal, in which I had performed ovariotomy up to the date of the publication of that work. I have performed it twice since, with a favourable result in both cases; making a total of 19 operations, followed by death in 7 cases, and recovery in 12. I feel confident that this mortality may be much diminished hereafter; but, as it stands, it is far better than the result of lithotomy in the London Hospitals, which ranges from 50 to 53 per cent. in patients over the age of 14. As the details of the following four cases have not been published, they may possibly be acceptable to the readers of the 'Medical Times and Gazette.'

Case 1.—On October 18, 1859, I met Dr. Rigby and Mr. Peirce, of Notting-hill, in consultation upon a single lady, only 17 years of age, who had been tapped eight times during the previous two years. She had been previously under the care of a physician who considered the disease to be ascites, but Mr. Peirce had recognised its true nature. It was arranged that this gentleman should tap in our presence,

which was done, and a careful examination was made after the largest cyst had been emptied. It was then found that there were several groups of smaller cysts in different parts of the abdomen, and looking to the frequency with which tapping had become necessary, and the condition of the patient, who was much emaciated, it was agreed to recommend ovariotomy. The patient and her friends being anxious to have it done, after the danger had been fairly put before them, it was arranged after a short time that I should perform the operation. Some delay was caused by a continuance of very foggy weather, but it was performed on November 19.

Dr. Priestley having administered chloroform, and Messrs. Curling and Peirce, and Dr. Sanderson, being present, and kindly assisting me, I removed the cyst and its contents, by an incision only four inches long, opening the cyst to the same extent, and breaking down the inner cysts, withdrawing the whole as they were emptied. Moderately extensive parietal adhesions had been previously broken down; and a portion of omentum was separated, in which were two vessels which required the ligature. The peduncle was secured by strong twine, and, with the omental ligatures, was kept outside the wound, which was united by hare-lip pins carried through the peritoneum on both sides, and by superficial sutures. The cyst and contents weighed thirty-eight pounds.

The after-treatment of this case was conducted with great ability and care by Mr. Peirce; and it is only necessary to add that rapid recovery followed, without one alarming symptom. An occasional opiate was given by the rectum, and hot linseed poultices were used for a few days all over the abdomen. The young lady is now in vigorous health, and was seen lately by Dr. Rigby, who was much struck by the contrast between the blooming girl before him, and the pale emaciated being who was the subject of our consultation only a few months before.

Case 2.—On Jan. 31st, 1860, I was consulted by a married lady from Manchester, aged twenty-six, the subject of ovarian disease of about three years' standing. Dr. Whitehead had tapped her in March and August, 1859, removing at the first time twenty-five, and at the second fifteen, pounds of fluid. A number of small cysts had grown rapidly between the first and second tappings. When I saw her she measured forty-one inches in circumference at the umbilicus, and nineteen inches from ensiform cartilage to symphysis pubis. She had been in pretty good health until the last six weeks, but had suffered much since from the distension, pain, and want of sleep. A portion of the tumour could be felt in the pelvis, between the rectum and the uterus, pushing the uterus upwards and forwards; but as the uterus was moveable, I hoped there were no pelvic adhesions of consequence, and, seeing no other resource open to the patient, advised ovariotomy.

I performed the operation on Feb. 6. Dr. Priestley administered chloroform. Dr. Markham, Mr. W. Adams, and Mr. Price were pre-

sent. The operation was precisely similar to that performed in the former case, the cyst having been emptied and drawn through a small incision. A group of smaller cysts moulded to the pelvic cavity were then also withdrawn through the wound, but the uterus followed the cysts closely, the tumour and uterus being connected together without any intervening peduncle. The Fallopian tube was much enlarged, elongated, and expanded over the growth, and adhering to it. I commenced the separation by the knife, dividing the Fallopian tube and part of the broad ligament, and secured some large vessels by ligature. The attachment lower down, towards the cervix and side of the vagina, was looser. Here I transfixed it, and tied in three portions, after securing it temporarily by a clamp. I found that the tissues included in the ligature could not be brought outside the wound without exerting a dangerous degree of traction on the uterus. Accordingly, after the wound had been united by hare-lip pins, I was obliged to be satisfied with bringing the ligature as near the peritoneal edges of the wound as I could, and leave sufficient opening for the escape of the decomposing stump.

She recovered well from the operation, and had some quiet sleep in the evening; the pulse, however, was 135, and rather feeble. Flatulence was rather troublesome, but there was no sickness. She seemed inclined to perspire, and said she was "comfortably warm." She passed a good night, frequently dozing, and perspiring a little; but in the morning the pulse was up to 148. She had no pain, but flatulence was troublesome, and she said she felt "tired." During the forenoon the pulse became feeble, rose to 160, and she continued to sink all day, although she took stimulants and beef-tea freely, and was not sick. No urine was secreted after three p.m. The intestines became enormously distended by gas, and she died at nine p.m., thirty hours after operation. No post-mortem examination was allowed.

I am disposed to attribute the death in this case partly to imperfect recovery from the shock of the operation and the consequent exhaustion, and partly to the absorption of some morbid product of the decomposing cyst. When a peduncle is secured outside the wound on the surface of the abdomen, the portion of cyst or peduncle strangulated by the ligature becomes quite putrid in a very few hours, and a black offensive discharge is generally very copious. The same thing must occur when the stump is within the peritoneal cavity, and the effects might be expected to resemble those produced by the injection of putrid substances into the veins. It is known that Dr. Clay thinks it better to leave the stump within the abdomen, and acts up to his belief; but, with two exceptions, I have always kept it outside,—this case, and one in which the patient recovered.

Case 3.—On May 26th I was requested to meet Dr. Ramskill and Dr. Buss, of Shoreditch, to decide upon the treatment of a patient, aged forty-one, the subject of a large multilocular ovarian cyst of two years' growth. She had not been tapped, and it was agreed that Dr.

Buss should tap her in our presence. Very little fluid was obtained at first, but by passing a uterine sound through the canula, and breaking down the walls of many inner cysts, a pailful of thick fluid was evacuated. She recovered well from the tapping, but filled again rapidly, and it was decided at a second consultation with the same gentlemen held on July 13th to perform ovariotomy, as she was already as large as before the tapping.

On July 17th I performed the operation. Dr. Echeverria administered chloroform. Dr. Ramskill, Dr. Routh, Dr. Drage, of Hatfield, Mr. Clay, of Birmingham, and Mr. Thompson, of Westerham, were present. The parietal adhesions were very extensive, requiring separation with the hand from almost the whole of the abdominal walls. A portion of omentum, also, had to be separated. A very little fluid passed through a very large trocar, so that I opened the cyst to the full extent of the wound, introduced my hand, and broke down the inner cysts while Dr. Routh and Mr. Clay withdrew them as they were being emptied. In this way the whole tumour was removed through an incision only just large enough to admit my hand, and little, if any, of the contents of the cyst entered the peritoneal cavity. There was some oozing of blood from the torn adhesions, and it was necessary to sponge out the pelvic cavity, but no ligatures were used. The pedicle was secured by twine ligature in three portions, and kept outside the wound, which was closed in the usual way by hare-lip pins and superficial sutures. The empty cysts weighed four pounds, the fluid collected thirteen pounds, and some eight or ten pints were lost.

The after-treatment was ably conducted by Dr. Ramskill and Dr. Buss. No medicine was given by the mouth. The abdomen was covered by hot linseed poultices, and when there was pain small opiate enemas were administered; but she only required three of these. Indeed, recovery was uninterrupted. As Dr. Ramskill said, "She had not even a dry tongue, a quickened pulse, any pain, or any heat of skin." The bowels did not act till the ninth day. On the next day the ligature separated from the peduncle, and on Aug. 11th, twenty-four days after the operation, the patient left town for Brighton by cab and railway, having been walking about the room for nearly a week before.

Case 4.—On Dec. 8th, 1859, I saw a single lady, aged thirty-six, in consultation with Dr. Rigby, and removed ten quarts of fluid from an ovarian cyst. She had not been tapped before. The growth was of about three years' standing. She suffered nothing from the tapping, gained flesh afterwards, but began to fill soon.

I saw her again on May 9, 1860. She then appeared about as large as before the first tapping. The girth was 39½ inches. It was arranged that I should tap again, and inject iodine if no secondary cysts were found. Accordingly, on May 21, I removed nine quarts of fluid; but, finding that large groups of secondary cysts had formed

on the left side since the first tapping, no iodine was injected. She remained rather feeble, with occasional nausea and faintness, for some days, but left town ten days after the tapping. She did not recover strength, complained of much headache at times, and lost flesh, while the size of the abdomen increased rapidly.

On July 13, consultations were held with Dr. Rigby, Dr. Tyler Smith, and Dr. Growse, of Brentwood. The result was that she determined to submit to ovariotomy, and I performed the operation at Brentwood, on July 28. The growth had increased so rapidly, she had become so thin and pale, had been so much distressed by the abdominal distension, and the pulse was so feeble, that at the last moment I consulted with Dr. Ramskill and Dr. Growse, as to the propriety of operating; but, fortified by their opinion that it was the only means of escape from speedy death, we determined to proceed, Dr. Ramskill carefully watching the action of the chloroform, Dr. Growse and Mr. Quennell of Brentwood, and Mr. Mason, assisting me. With the exception of one small band of adhesion at the site of the previous tappings, the cyst was quite unattached. This band readily yielded to the hand. The principal cyst was tapped, emptied, and partly withdrawn. But large groups of inner cysts prevented the escape of the whole tumour through the small incision. I therefore passed my hand into the large cyst, and by breaking down the smaller ones and squeezing out their contents, the whole tumour easily passed through an incison only just large enough to admit my hand. Dr. Growse and Mr. Quennell were so careful in drawing out the cyst as it was emptied, and protecting the edges of the wound by hot flannels, that not a drop of the contents of the cyst entered the peritoneal cavity. There was rather free bleeding from the broken-down adhesion, but it subsided spontaneously. The peduncle was long, but very broad, extending along the brim of the pelvis on the left side. It was secured by transfixing and by a twine ligature. The wound was closed in the usual manner, and the same plan of after treatment agreed upon. It was conducted most ably and attentively by Dr. Growse, and I have little more to record than gradual recovery. For three days she remained very weak, the pulse ranging from 104 to 130, the skin inclined to be hot, and there was a good deal of flatulence with occasional vomiting. But there was very little pain. She slept very comfortably after an occasional opiate enema, and all anxiety was over by the fourth day, when the ligature on the peduncle had separated, and the deep sutures were removed. The superficial sutures were left till August 4. I have since had most favourable accounts of her progress, and look upon convalescence as established. Her sister wrote to me on August 21, twenty-four days after the operation, saying:—"My sister walked into the front room yesterday, where we dined and spent the day. She is now sitting in an easy-chair, *at work*."

I am anxious to direct special attention to the simple after-

treatment pursued in these cases,—as I am convinced that in many successful cases the fatal result has been principally due to over-active or meddlesome medication. The published reports of more than one case clearly point rather to opium than to ovariotomy as the cause of death. In other cases stimulants seem to have been given in mischievous excess; while the opposite treatment by starvation and purgatives is not yet absolutely exploded. I have made a great number of experiments on dogs, rabbits, and guinea-pigs, in order to determine the best mode of uniting wounds of the abdominal parietes; and nearly the whole of the animals operated on recovered,—yet the rabbit is peculiarly liable to peritonitis and the guinea-pig is in about the same degree as in the human subject. But, although no after-treatment whatever was adopted, and the animals were left with their ordinary supply of food, and to their natural instincts, they recovered with hardly an exception. This experience has led me to regard perfect rest in a well-ventilated room, comfortably warm bedding, and extreme cleanliness, while simple food and drink are given in small quantities, as the patient's feelings suggest, as the principal things to be attended to. The application of warmth and moisture to the abdomen is very pleasant to all such patients, and undoubtedly useful. Opium in sufficient doses to relieve pain is also of great use; but in larger quantities it is unnecessary and injurious. If given by the mouth in any form it is apt to be followed by vomiting, but this evil is in a great degree avoided by injecting it into the rectum. Here also it seems to have a local soothing effect; especially in relieving irritability of the bladder, which is sometimes troublesome. Purgatives on the one hand, or artificial constipation by opium on the other, seem to be equally mischievous. Over-excited peristaltic action may produce peritonitis directly, while the opposite condition may do so indirectly, and certainly leads to distressing flatulent distension of the intestines. It seems to be a perfectly safe rule to allow both stomach and intestines to adapt themselves to their altered circumstances with very little, if any, medical interference, and certainly rather to do nothing than to run any risk of doing harm.—*Medical Times and Gazette, Aug.* 25. 1860, *p.* 178.

105.—CASE OF TUMOUR IN THE ABDOMEN WITH A HISTORY OF SUPPOSED PREGNANCY.

(Under the care of Dr. JAMES JONES, Metropolitan Free Hospital.)

[The following case is one of great interest. It bears more especially on the question of diagnosis, and indicates the necessity for extreme care in arriving at a conclusion as to the existence of a dead fœtus in the abdomen.]

Mrs. W., a fairly healthy woman, aged twenty-nine, was admitted under Dr. James Jones' care in June last. She was the mother of

one child now nearly three years old. The history which she gave of herself was, that after the recommencement of menstruation on the weaning of her infant, she continued to be unwell regularly, until Jan. 1859. At the latter date the menses were suspended, and she believed herself pregnant. In due time she felt the motions of the child, her abdomen enlarged, her breasts increased in volume, and her whole condition was such that she never entertained a single doubt as to her condition. In Oct. she had attained a size about equal to that of the latter months of her previous pregnancy. A week or ten days after the time at which she had expected the birth of her child she was attacked by pain in the abdomen, and indications of the commencement of labour. The midwife whom she had engaged to attend her was summoned, and remained in attendance for three or four days, when, as things did not progress, a consulting accoucheur was called in, who, with some hesitation, expressed his belief that the patient was not pregnant, and that an abdominal tumour existed. After this the woman got about again, and in November she received a blow on the abdomen subsequent to which she never again felt what she had considered to be the movements of the fœtus.

Condition at the time of Admission (eighteen months after the suspension of menstruation, and eight after the supposed death of the fœtus).—The woman's abdomen was about as large as that common in the seventh month of pregnancy. It was occupied by a large firm mass, which was centrally placed, and could be moved a little from side to side. In this mass the most careful handling failed to discover any irregularities like those presented by the limbs of a child. Over its right side at its upper part there was a broad shallow furrow, but with this exception its surface was equally rounded and equally firm and unyielding. The tumour did not deviate to either side, and occupied the whole of the front of the abdomen from the pubes to midway between the umbilicus and ensiform cartilage. Her breasts were both of them of considerable size, and she could squeeze milk from the nipples. The areolæ were dark. A broad brown streak extended from the umbilicus to the pubes. On examination per vaginam the os uteri was found to be high up, the cervix was short and appeared to end by being expanded into a rounded solid mass which occupied the upper part of the pelvis. With some difficulty a flexible uterine sound was introduced for the length of four inches.

Here, then, was obviously the question of diagnosis between a fibrous tumour of the uterus and a defunct fœtus, the result of an extra-uterine or parietal gestation. The woman's history, and the fact that she had had no hemorrhages, were strongly in support of the latter hypothesis, and several of those who examined her were inclined to adopt it. Against such a view of the case, however, was the whole of the evidence derived from the examination of the tumour, whether through the abdominal wall or by the vagina. It should be stated that the woman was thin, and that the tumour could consequently be

very satisfactorily handled. Then, again, as to history. The suspension of menstruation was the only fact to which much real weight could be attached. Cases of fancied pregnancy, on the part even of those who have previously borne children, and might be expected to have had experience enough to prevent such a mistake, are yet common enough. It is well known, also, that the breasts will enlarge in sympathy with any uterine tumour, and even that milk may be secreted under such circumstances. Although also it is perfectly true, that the growth of most intra-uterine tumours, more especially in their earlier stages, is attended by attacks of hemorrhage, yet exceptions to this are far from unfrequent. Thus, therefore, the history, though at first sight appearing clear and definite in its indications, seemed to lose its conclusiveness on more searching scrutiny. Relying upon the more trustworthy evidence afforded by actual examination, Dr. Jones and most of his colleagues were induced to form a very positive opinion that the tumour was in reality a fibrous one, and had nothing to do with misplaced gestation. —*Med. Times and Gazette, July* 21, 1860, *p.* 58.

106.—ON THE DIAGNOSIS OF TUMOURS OF THE BREAST.

By JOHN ERICHSEN, Esq., Professor of Surgery and of Clinical Surgery in University College, &c.

[In the present article, Mr. Erichsen treats principally of the diagnosis of cancer of the breast from cystic and adenoid tumours of that organ.]

These cystic growths, though not so common as cancer and the other solid tumours, are yet of by no means infrequent occurrence. They are of three distinct kinds:—1. Those in which the tumour consists of a single unilocular cyst; 2. Those which consist of several independent cysts aggregated together into one tumour—multilocular cysts; 3. Those in which a series of small cysts are diffused through a fibrous or hypertrophied mass; in fact, a combination of cysts with a chronic mammary tumour. Two theories are in vogue as to the origin of this form of cystic development. According to one set of pathologists, it is produced by the obstruction and subsequent dilatation of a lacteal duct. But this theory, I think, is weak: from the fact of our not being able to trace one of these ducts into the cyst; from the fact that the fluid contained in these cysts shows no trace of lacteal origin; and from the fact that such cysts are met with elsewhere, in places where no lacteal duct previously existed. The other theory is, that these cysts are entirely new formations; and this, partly for the reasons before mentioned, and partly because these cysts closely resemble those met with in other secretory glands both as to structure and contents, appears to me to be the more tenable of the two.

These tumours, I must premise, whether unilocular, multilocular, or consisting of cysts diffused in a mass of fibrous or hypertrophied gland-tissue, are always composed of thin walls, formed of condensed cellular tissue, and containing in their interior a fluid variable in amount and character; being in one serous, in another glairy, and in a third bloody; it is very commonly of a light straw colour, not unfrequently it is brown, and sometimes sanguineous, but these differences are accidental and of no importance.

We will now consider the diagnosis of these cystic growths from cancerous and other solid tumours of the breast. And, first, with regard to the unilocular cyst: this is the most common form, and occurs generally in women at what one might call the "cancerous age," viz., forty-five to fifty; it is frequently referred to pressure, or to a blow on the part, or it may be connected with uterine disturbance at the period of the cessation of the menses. Thus so far as the age and proximate cause are concerned, the history throws comparatively little light on the subject, and the diagnosis has therefore to be made entirely by palpation and examination of the tumour. Now you can easily conceive that, depending only on manipulation to form a correct judgment of the nature of the tumour, the surgeon may be exceedingly liable to form an erroneous opinion. I could relate to you many cases in which error of diagnosis has occurred, but I will confine myself to a few of the more illustrative.

I was requested some time ago, by my friend, Mr. Walter Wilson, to see a married lady, aged 45, who had in the right breast a tumour of about the size of an apple, hard and painful on pressure. This, she said, had been diagnosed as a fibrous tumour by a surgeon in the country, who had recommended her to come to London and have it removed. On examining the tumour I suspected it to be a cyst. It had not the stony hardness of a solid tumour, but felt obscurely elastic on deep pressure. Acting on this supposition, I introduced an exploring trocar and let out about two ounces of dark serous fluid; the cyst never refilled, and the patient went home perfectly cured.

I was requested one day to see an unmarried lady, aged 44. On going to the house I found her bathed in tears, and her sisters in great distress around her. I was told she had a tumour of the breast, which had been pronounced to be a cancer. On examination I found a tumour in the right mamma, about as large as a pigeon's egg. It had been noticed about fourteen months previously, and the patient had been under both medical and surgical treatment for it. It was rounded, circumscribed, situated at the outer and upper part of the gland, hard but smooth, and not heavy to the feel. On close manipulation, I felt some elasticity. I told the patient that I did not think it was a cancer, but a cyst, and that if I punctured it and drew off the fluid contained within it she would probably have no further trouble. She then informed me that she had seen an eminent surgeon, who had pronounced it to be cancer; had explained that an

operation was necessary; that the whole of the breast must be removed, and had fixed an early day for its performance. I requested to meet this gentleman, but this was refused by the patient and her friends; and as our opinions differed so widely, it was agreed that she should have the benefit of Sir B. Brodie's opinion. That distinguished surgeon saw the case with me the next day, and, he having acquiesced in the opinion I had expressed, I tapped the cyst, and let out about an ounce and a half of yellowish serous fluid. The tumour collapsed, all idea of operation was abandoned, and the patient has continued well up to the present time.

I saw the other day a very similar case. It was that of an unmarried lady, aged 48, who had had for about twelve months a tumour in the left breast, which had gradually increased in size, until it had attained the magnitude of a Tangerine orange. It was hard, circumscribed, situated at the axillary border of the gland; it had also been pronounced to be scirrhus. But, suspecting from its elasticity that it was cystic, I punctured it with an exploring trocar, and drew off about ten drachms of clear fluid, causing the immediate subsidence of the tumour.

Now here were three cases of simple cystic or fluid tumour of the breast, which had erroneously been pronounced to be solid, and which would have been submitted to amputation of the mamma if the mistake had not been discovered in time. It is of very great consequence, therefore, to be cautious in these cases in pronouncing a definite opinion, and to neglect no means in perfecting your diagnosis. How is this to be done? As I have already stated, the history very often throws no light on the nature of these cases. Cysts occur at the same age and among the same class of people as cancer of the breast, but there is one circumstance of great importance to which you must always attend, and that is the presence or absence of *elasticity*. This last character may be said to be the great diagnostic point between these tumours and cancer; and whenever you feel, or even suspect, elasticity, you ought to introduce an exploring trocar. If the tumour is cystic, the fluid escapes, and you probably hear no more of the case. But if, on the contrary, it is solid, a drop or two of blood only oozes out, the puncture soon closes, and no harm is done. In making this puncture, there is one little point to be attended to, and that is, if you use an exploring needle, take care to push it straight in, and not to make the puncture in any way valvular; for if you do, the fluid may not escape, and thus a serious mistake may arise; it is, however, far better in all cases to use the exploring trocar in preference to the grooved needle.

If this little operation—tapping—does not procure the closure of the cyst, you must resort to other measures, such as injecting it with iodine, introducing a seton, or, if these fail, excision of a piece of the cyst-wall. But you will generally find that tapping and the subsequent use of iodine lotions will suffice to effect a cure.

The next kind of cyst—the *multilocular*—is more difficult to diagnose than the unilocular; firstly, because it is generally seated deeply in or beneath the gland, while the unilocular occurs chiefly at the border or anterior surface, and because there is not so much fluctuation, owing to the fluid being divided among several cysts. But yet there is that never-failing sign of cystic disease—elasticity. You will find, however, that there often exists a good deal of condensed fibrous matter round about these tumours, and hence the removal of the whole gland may be required, the extirpation of the cysts alone being impossible. One reason why cyst-tumours are often so difficult to diagnose as such, is that they are often associated with cancer. There great difficulty exists, especially in the early stage; and these cases are exceedingly liable to be confounded with cystic sarcoma, and indeed in some cases you cannot make your diagnosis until the tumour is removed, and then only by a careful microscopic examination.

A woman, aged 45, was admitted into this hospital with a tumour of the size of an orange, situated in the right mamma deeply over the pectoral muscle. It had existed for about five years, was not adherent to the skin, and there were several cysts of the size of plums. At the upper part of the mass, which could be felt through the lately mucous covering, the nipple was not retracted; there was only one slightly enlarged gland in the axilla. Now here were all the characters of a "cystic sarcoma"—slow growth, no adhesions, cysts, and no material glandular implication; and yet, after the removal of the breast, the microscope revealed the tumour to be distinctly and decidedly cancerous.

You will sometimes find that tumours of a mixed, possibly a semi-malignant character, occur with cysts, and give rise to great difficulty in the diagnosis of their exact nature, and render it impossible for you to pronounce with certainty whether they are benign or malignant. The following is a case of this kind.

An unmarried lady, 40 years of age, and in excellent health, was sent up to me by my friend Mr. Tuxford of Boston, last November, with a tumour of the right breast, which without assignable cause, had commenced growing about five years and a half ago; it increased slowly until it had attained the size of an orange about two years since, but afterwards much more rapidly, until at last it reached the size of an adult's head. The skin covering it was not reddened, and, though thinned, was neither adherent nor infiltrated. There was no pain at night, or after handling it; and no enlarged glands could be felt in its neighbourhood. The superficial veins were much enlarged over the tumour, and a good sized artery was felt to pulsate over its upper part. It was elastic, lobulated; and a large mass, of a cystic character, projected from its anterior surface. There were no adhesions between it and the pectorals, or to the skin. The operation was performed on November 10th. On removal, I found that the tumour weighed ninety-six ounces, and consisted of large encapsuled masses,

of a dull grey or brown colour. On section, it was solid in parts, and infiltrated with a gelatinous fluid; in others, there were large cysts, containing several ounces each of dark stringy mucoid fluid. Dr. Harley, who examined it, pronounced it to be colloid. There were also masses of fibroplastic deposit in some parts.

Now here was a case that approached closely to malignancy in its local characteristics, without any constitutional symptoms of cachexy; and which presents an appearance on examination that renders it doubtful whether it may or may not yet recur.

The next class of cases to which I would direct your attention are the chronic mammary or *adenoid* tumours. These are of exceedingly common occurrence; and there are two or three conditions with which they are often associated, the knowledge of which materially assists the surgeon in his diagnosis. 1. With regard to the age, they generally occur before the cancerous age, in early womanhood, between the ages of 18 and 25. 2. They are almost invariably slow in their progress. 3. They are lobulated, distinct, non-adherent to the skin or pectorals, and circumscribed. 4. There is no cachexy or glandular enlargement; and, in fact, they appear to be quite local and benign. There is usually no cause to which they can be assigned; but I believe that they frequently occur in young women of nervous temperament, and are commonly associated with uterine disturbance of some kind. The chief diagnostic points are, therefore, the age of the patient, the slowness of growth of the tumour, and the perfect freedom from constitutional symptoms; but yet any one of these conditions may be present in cases of undoubted cancer of the mamma.

Scirrhus very rarely occurs at the early period at which the adenoid tumour is common; but yet it is occasionally met with in young women. Some years ago, I removed in this hospital a tumour from the right breast of an unmarried female aged 23. It was as large as a small flattened orange, was hard, nodulated, but not adherent to the skin. There was no retraction of the nipple, and it had been growing for about eight months. No cause could be assigned for its appearance, and it was supposed to be adenoid. However, on account of the large size of the tumour, as compared with that of the somewhat atrophied mamma, I removed the whole of the breast, together with the tumour; and it was well that I did so; for, on microscopic examination, it was found to be scirrhus. The patient subsequently married; and when I last heard of her, two years after the operation, she was in good health. Now, here is a case in which cancer occurred within the period usually assigned to chronic mammary tumour; and hence you cannot rely altogether on the age of the patient as a means of diagnosis.

Next, with regard to the slowness of growth: although it is an undeniable fact that benign tumours usually grow slowly, and that rapidity of growth is generally a sign of malignancy of action, yet this rule must also be taken with exceptions, as in the following case:—

Last June, I was requested to see an unmarried lady, aged 40, of a nervous sanguineous temperament, who had been in bad health for many years, suffering from uterine disturbance, dyspepsia, and latterly from pulmonary symptoms. At the age of 18—that is, twenty-two years previous to my advice being sought—she noticed for the first time a small tumour in her left breast; this continued perfectly stationary, and about the size of a pigeon's egg, until last February, when it for the first time became painful, and began rapidly to enlarge. Being at this time in Italy for the benefit of her health, this lady saw two distinguished Roman surgeons, who, after careful examination and exploratory punctures, pronounced the tumour to be "myeloid," and recommended extirpation of the mamma. The patient, however, preferred to return to England, and have the operation performed here. On her way home, she saw Velpeau in Paris, who, with great accuracy of diagnosis, pronounced the tumour to be "benign," and gave her a written statement to that effect. When I saw her, on her return to this country, in June last, I found a tumour of the left mamma, perfectly mobile, firm, solid, inelastic; it had attained the size of an adult's head, and was rapidly increasing. There was no glandular enlargement in the axilla; the skin was thinned and reopened, but not adherent; the superficial vessels were much enlarged, a tortuous network of veins and one or two large arteries running over the tumour. There was no constitutional cachexy; but the patient's health and strength were at the lowest ebb, from general and long-standing constitutional derangement; and she could not sleep at night, not so much from pain (which, however, was constant, and at times very severe), as from the constant anxiety of mind which the presence of the growth produced. Sir B. Brodie and Dr. Walshe, who saw the case in consultation with me, both agreed that it was probably benign, but that no operation was practicable until the patient's state of health was improved. However, by attention to diet, and by being put on a proper plan of treatment, this was so much ameliorated that, in July last, I was enabled, with the assistance of Mr. Marshall and Dr. Cowan of Glasgow, to perform the operation for its removal. This was attended by no difficulty, and by very little hemorrhage, notwithstanding the size of the tumour, which weighed rather more than five pounds. The patient made an excellent recovery, and was able to leave town in less than three weeks. On examination after removal, the tumour was found to be lobulated on its surface. The section showed it to be homogeneous, and of a uniform greyish colour, with no soft points or cysts, but distinctly and firmly encephalous. Dr. Harley, who examined it microscopically, found it to be a specimen of the chronic mammary tumour of Sir Astley Cooper. In the plastic matter taken from different lobules, examples of the glandular tissue were found. Some of the blind tubes were remarkably distinct, and well filled with cells. There was no trace of cancer.

Now here was a tumour which, after remaining of small size and stationary for more than twenty years, suddenly, and without obvious cause, began rapidly to increase; so much so that, in less than six months, it had increased in size from that of a pigeon's egg to the magnitude of a mass weighing more than four pounds. Here was extreme rapidity of growth without malignancy of character. In fact, this extreme rapidity of growth resembled rather what is not unfrequently found in encephaloid disease, than what we expect to meet with in the chronic mammary tumour, and rendered the diagnosis not a little difficult; the more so, as it was possible that the chronic mammary tumour, which had existed for so many years in a stationary and passive condition, might have suddenly undergone malignant degeneration, and thus taken on rapid increase of bulk. This, however, was disproved by the careful microscopical examination made by Dr. Harley, who found that the tumour did not present a trace of malignant structure.

It is, however, important to bear in mind that cystic and adenoid tumours may remain for a long time in a benign and passive state, and then assume malignant characters. This happened in the case of a woman aged 48, who was sent to the hospital by my friend Mr. Adams two years ago. At the age of 27, she had first observed a tumour in the left breast. This had slowly increased in size, until it had attained, at the end of fifteen or sixteen years, the size of the fœtal head. When I first saw it, in January 1858, one of the cysts of which it was composed had ulcerated, and a thin sero-sanguineous discharge oozed out of the opening. The general health was good. There was no glandular enlargement in the axilla, or adhesions of the skin, except around the ulcerated parts. It was freely movcable on the pectorals. An operation for the removal of the tumour was proposed, but this the patient refused to consent to. At the end of six months, she returned with a large ulcerated cavity in the centre of the tumour, and fungating masses sprouting from the bottom of it. There was still no cachexia, or enlarged glands in the axilla. The patient now consenting to an operation, I extirpated the whole mass with the mamma. On examining the tumour after its removal, it was found to be cystic. There were several large cysts, of the size of pigeons' eggs, containing turbid but light coloured serous fluid. The central portion of the tumour, and that at the base of the fungus, were solid, grey, and rapidly undergoing softening and disintegration. On squeezing the portion of the mass (the base of the fungus), a milky juice exuded; and Dr. Harley who examined the tumour, stated it to be encephaloid. The sarcomatous structure beyond this, constituting the general mass of the tumour, appeared to be adenoid. The surface of the fungus, when exposed and protruding beyond the cyst, was epitheliomatous.

Now here were cystic sarcoma, encephaloid, and ephithelioma, associated in one growth. The encephaloid was stated by Dr. Harley to be cellular, without fibres, showing rapid development. The epithe-

lioma was confined to the surface of the fungus. The history of this case, the very lengthened period (more than twenty years) that the tumour had existed, the absence of all constitutional cachexy, of deep adhesions, or of glandular enlargement or other secondary deposits, and its appearance only six months previous to removal, all pointed to primary simple cystic disease of the mamma, in which, as the result of secondary changes, encepheloid had developed itself; being an instance of the conversion of a simple into a cancerous tumour of the breast. This patient died about a twelvemonth after the operation, of gangrene of the foot and disease of the heart. The cicatrix was quite sound; but in the substance of one of the ventricles a nodule was found, which was considered by those who examined it to be of a cancerous nature.—*British Med. Journal, April* 14, 1860, *p.* 279.

107.—RECURRENCE OF AN ADENOCELE OR MAMMARY GLANDULAR TUMOUR AFTER THREE SUCCESSIVE REMOVALS; ULTIMATE CURE.

(Under the care of JOHN BIRKETT, Esq., Surgeon to Guy's Hospital.)

A thorough acquaintance with the various tumours involving the mammary gland is essential to the forming of a correct opinion as to their malignancy and liability to recurrence. When growths appear at an early age—say in women under thirty—the probabilities are in favour of their benignancy, and that they pertain to the class of chronic mammary, mammary glandular, sero-cystic, or adenoid tumours, as they have been described by Sir Astley Cooper, Sir Benjamin Brodie, Mr. Birkett, and other writers. A tumour met with in the breast of a young girl, for instance, is most commonly of this nature, although occasionally there are exceptions; and when once removed, they never return. Such examples are by no means rare; many have appeared in our "Mirror." We examined two on the 17th inst. at another hospital: one in a girl of fourteen, with a small adenocele of the right breast; another in a young woman aged twenty-seven. These patients had applied for advice under the impression that their tumours were cancerous; their true nature was, however, readily made out. In the latter part of 1859, we saw three examples in young patients, under Mr. Birkett's care at Guy's Hospital, all successfully operated upon. One of these was especially interesting, because both breasts contained a tumour; that in the lower part of the right had been growing for three or four years, and was always painful before the appearance of the catamenia; and some three or four months previous to her admission into the hospital, several small growths were developed in the left breast. The one occupying the right gland was alone removed on the present occasion. A similar form of tumour,

developed during pregnancy, in a young married woman, we saw Mr. Birkett excise in October last; it had prevented suckling, and was deeply seated behind the nipple. A good recovery ensued, without damage to the proper glandular structure.

That these simple or innocuous tumours, nevertheless, do occasionally recur, we have clear evidence to prove, and the following instance is an example. There is a point of some importance in the case, moreover, not to be overlooked—viz., that although the primary disease resembled the chronic mammary tumour, yet the age of the patient was greater than that at which the malady usually shows itself; and after three removals it recurred a fourth time, the extirpation of the entire gland having been followed by perfect success thus far. Curiously enough also, although the glandular elements were observable on each occasion, yet the tumours varied in their characters, being at one time mammary-glandular, and at another cysto-sarcomatous. In a former "Mirror," ('Lancet,' vol. ii., 1857, p. 114,) we have recorded an instance in which all the varieties of tumour belonging to the class Adenocele were developed in a single breast. Its removal, however, was followed by recurrence (idem, p. 548) within four months, in consequence of the nipple having been left behind.

The case of this patient, now fifty-three years old, is reported in Mr. Birkett's paper, on Adenocele, in 'Guy's Hospital Reports' for 1855, p. 161.

The facts are as follows:—The patient was forty-five years old when the first growth was discovered. This growth had existed eleven months when removed. One year and ten months elapsed before the discovery of the second tumour, which was excised three months subsequently. In the course of eight months a third growth made its appearance, and was removed four months afterwards. All the growths showed the elements and tissues of the mammary gland structure; but the first and last most closely resembled the "chronic mammary tumour" of Sir Astley Cooper, whilst the second belonged to the class "cysto-sarcoma" of other pathologists.

This case and another are related to prove the alternation which may occur between these new growths in the same individual at different times, as well as the probable identity in their physiological relations.

In November, 1856, the patient showed Mr. Birkett a fourth very small growth near the cicatrix, but on the axillary part of the breast, which had not been taken away. She had then been free from recurrence one year and eight months. During 1857-8, and the early part of 1859, scarcely any increase in the tumour took place; but in the spring and during the summer it rapidly enlarged, and at the time of removal measured about four inches in the largest diameter.

This fourth growth was excised August 23rd, 1859, together with every particle of the normal breast gland-tissue which could be found. It consisted of the same elements as those composing the former

growths; but in this instance it showed, with the happiest effect, a combination within the same envelope of the "chronic mammary tumour" of Sir A. Cooper, and the "cysto-sarcoma:" for in some parts were firm, fibrous, solid gland-like masses; whilst in others, cysts with the characteristic fluid and intra-cystic growths, met the eye. The patient suffered very little during the healing of the wound, which was nearly complete in about a fortnight, and the patient left the hospital quite well.—*Lancet, July* 28, 1860, *p.* 81.

108.—SINGULAR CASE OF INVERSION OF THE URINARY BLADDER DURING PREGNANCY.

By Henry Johnston, Esq., L.R.C.S.I.

On the evening of Sunday, Aug. 8th, 1859, I was requested to visit Mrs. M., aged twenty years, and was informed that she was threatened with a miscarriage. I found her apparently suffering very intensely from "pains" attended with such tenesmic efforts as to induce her to remain almost constantly on the night-chair. I was told that there had been considerable hemorrhage with discharge of "waters" the previous day. She had been nine months married, and calculated that she was rather more than four months pregnant. For weeks past she had suffered from irritability of the bladder, but enjoyed very fair health until Friday, the 6th, when, after over-exerting herself in lifting a box, she felt great uneasiness in her back, attended with frequent and urgent desire to pass water. These symptoms became more urgent on Saturday, and were accompanied with severe attacks of vomiting. She was unable to retain the contents of the bladder; the tenesmus was most distressing; and she had no relief except when on the night-chair. It was thus I found her on my first visit; and after considerable persuasion I succeeded in getting her to retire to bed. On my attempting to make a vaginal examination, she became very impatient, and complained very much of the intense pain I gave her, far exceeding that commonly given in a vaginal examination. This did not escape my notice. On examining I found a soft fluctuating tumour, in size about that of a pear, pyriform in shape, extending as far down as the os externum of the vagina, and feeling very like a bag of membranes. My finger passed from the base of the tumour along its narrow neck-like portion, into a small narrow cavity, which I considered was the dilated os uteri, being also under the impression that the narrow neck-like portion of the tumour was the cord attached to the ovum. I followed up this cord into a cavity, which I presumed to be that of the uterus. My patient now insisted on being allowed again to remain on the night-chair, and complained of her inability to bear any further examination. Being under the impression that the uterus would expel its contents, and there being no hemorrhage, I gave some general directions and returned home.

I visited her on Monday morning, and found her in the same state of suffering; and having slept none during the night, her pulse was now excited, and the attacks of vomiting frequent. She was unwilling that I should make any examination. I therefore administered an anodyne, and returned in the evening with my friend Dr. Bryce, to whom I communicated my impression that my patient was going to miscarry. He examined the tumour; but in making his examination discovered that he could pass his finger up behind it, into the vaginal canal, where he felt the "os uteri" still undilated. He felt puzzled to explain matters, and at first suggested the idea that there was a malformation. On further consideration he concluded that the tumour was a vaginal vesicocele—a prolapsed bladder—and not a bag of membranes.

Dr. Bryce's discovery of the vaginal canal, and of the os uteri still undilated, was certainly new light to me. I again made an examination of the tumour; in doing so I found that it yielded to gentle pressure, continuing which, it retired upwards, through the cavity which I had supposed to be the os, into the cavity which I had supposed to be the uterus, but from which flowed a small quantity of urine. The true nature of the case was for the first time manifest, viz., that it was one of inversion of the bladder, that the os through which I passed the tumour was the "meatus urinarius," dilated to such a capacity as to admit two of my fingers. I suggested this explanation to Dr. Bryce, and, in order to satisfy ourselves on this point, we made an ocular inspection of the state of the meatus urinarius, which confirmed the diagnosis. Immediate and decided relief followed the reposition of the inverted bladder: the vomiting ceased; the pulse fell; our patient's expression changed; the pain in the back ceased; the urgent symptoms of irritation of the bladder were almost quite relieved; and she soon fell asleep. We visited her some time afterwards, and found that there had been no return of the inversion. I drew off a quantity of bloody urine with the catheter. We enjoined her to employ no voluntary efforts in micturating, for a time; administered an anodyne; and ordered an oil draught the following morning.

The next day I found her comparatively easy. During the next month, however, she suffered from incontinence of urine, even when sleeping; and complained very much of a scalding sensation in the urethra. The urine was bloody, and there was a considerable deposit of mucus. These symptoms were treated, and gradually disappeared. I saw her last on the 3rd of December. She was then quite free from any vesical uneasiness; was able to retain her urine as usual; felt in perfect health; and was soon expecting her confinement.

Believing that medical science may be advanced by the publication of our mistakes, as well as of our successes, in practice, I have given a fair and candid account of this peculiar, and I believe I may add uncommon, case. Prior to my meeting with it I had never heard of such

an accident. I was, therefore, quite unprepared for it, and did not recognise it. It is well that it should be recorded, in order that it may enable others to exercise caution in dealing with tumours of an obscure character, and attended with perplexing and anomalous symptoms. We should ever be prepared to meet with cases presenting symptoms and features for which we may have no precedent. It may appear strange that I was so slow in recognising the true nature of the case, and that in the first instance I made a very decided, and what might have been a very serious, error in diagnosis (as, owing to the view I first took of this case, I contemplated puncturing the membranes). I had been led to believe that Mrs. M. was about to miscarry. My mind was pre-occupied with that idea; and after I examined I felt so satisfied that the tumour was the ovum entering and protruding from the os that I did not see any necessity to push my external examination, and my patient was certainly very unwilling that I should do so. To Dr. Bryce belongs the credit of throwing that light on the case which led to the recognition of its true nature. I have been unable to discern any similar case on record, viz.—inversion of the bladder occurring during pregnancy.

Dr. Meigs, in his work on 'Diseases of Women,' details the history of a case of inversion of the bladder at the orifice of the urethra, in the person of a little girl three years of age. In her case, it was mistaken for a vascular tumour, and was about being ligatured when its true nature was fortunately discovered. Dr. Murphy had a similar case in Dublin, an account of which I have been unable to find. Jones and Sieveking, in their 'Pathological Anatomy,' mention the possibility of introversion of the bladder, and even, in females, of its projecting from the meatus urinarius externally. We find, therefore, that inversion of the bladder is an accident which we should be prepared to meet, and that as it occurs particularly in children, or in pregnant females, we should be careful to distinguish it in the former from a vascular tumour, and avoid ligaturing it; in the latter from a bag of membranes, and avoid puncturing it; as in either case fatal results would be almost certain to follow. I would only suggest whether we might not mistake inversion of bladder for polypus? Since this case was read, Professor Simpson has drawn my attention to two cases of inversion of the bladder, reported in 'Cruveilhier's Pathological Anatomy,' one occurring in the person of a male, the other in that of a female.—*Dublin Hospital Gazette, April* 16, 1860, *p.* 120.

109.—*On the Treatment of Vaginitis and Superficial Inflammation of the Neck of the Uterus by Tannin Ointment.*—Dr. FOUCHER has observed that tannin combined with lard is an excellent local application for vaginal inflammations; and that ointments are preferable to injections, because they remain better in contact with the inflamed mucous membrane. In cases of simple vaginitis, he intro-

duces every morning, by means of the speculum, into the vagina where it is in contact with the neck of the uterus, a large piece of wadding smeared with a thick layer of tannin ointment. A thread is attached to this wadding, so that the patient can remove it herself. Dr. Foucher has employed the same treatment for leucorrhœa, and he states that it has been successful, although in many cases constitutional remedies are necessary in addition to the local application.—*Brit. and For. Med. Chir. Review, July*, 1860, *p.* 280.

110.—CLINICAL REPORTS OF SOME DISEASES OF THE RECTUM, &c.

By I. Baker Brown, Esq., F.R.C.S.

[The present paper gives some cases showing the possibility of affections of the rectum producing certain symptoms which have been ascribed to diseases of the uterus. In the author's work on 'Surgical Diseases of Women,' the opposite condition is shown, i.e., that the uterus may give rise to symptoms from the rectum.]

Case. 1.—L. P., aged twenty-five, single, admitted into the London Surgical Home, Dec. 22nd, 1858. She complained that she had suffered for a long time from heat and pain in the womb, with pains in the back, and a general feeling of uneasiness in her bowels. She looked ill and worn, and was suffering much from dyspepsia. She had been treated by leeches to the uterus, for dyspepsia, and for the uneasiness in her bowels, but without deriving the least benefit. On examining the uterus, no disease could be found; menstruation was regular; there was slight leucorrhœa. On inquiry if she had more pain at the time of, or after, an action of the bowels, she replied, "Yes, always; that then she had a sharp shooting pain darting through the womb, and that she was obliged to lie down because the pain and uneasiness were so great in her bowels; in fact, that she dreaded going to the water-closet." On passing my finger into the rectum she complained of acute pain, and I found a deep fissure just within the sphincter, and opposite to it a small pendulous polypoid body, the pea-like end of which dropped into the fissure.

Treatment.—A dose of castor oil was ordered early in the morning of the following day, and after it had acted freely the rectum was well washed out with warm-water enemas; the fissure was then divided by Copeland's blunt-pointed straight bistoury, the polypus tied, and the rectum plugged with lint soaked in sweet oil. Two grains of opium were given, and generous diet ordered.

Dec. 25th. The bowels were opened by a castor oil enema; afterwards the nurse applied sweet oil on her finger to the whole cut surface, and repeated it once daily.

Jan. 6th, 1859. Discharged, quite cured. I have heard of her since as continuing perfectly well.

Practical remarks.—This case well illustrates the proposition which I have just advanced. I would also wish to observe that a very large number of fissures of the rectum are produced by these little polypoid bodies, as they will be found in almost every case if carefully sought for. It will be observed that the dressing of lint and oil was never repeated. This has been my invariable practice for the last twenty-five years, having been taught the great practical fact by my esteemed friend, the late Dr. Copeland, that it is never necessary to interfere with the parts by the painful process of reintroducing the lint, if care be taken that the first dressing be left in for forty-eight hours; after that time there is no fear of union by first intention, but, on the contrary, a healthy granulating process is set up, which continues till the end.

Case 2.—Emma C., aged forty-two, married, mother of eight children, admitted into the London Surgical Home on the 11th of June, 1859. She complained of having suffered for a long time from pain and bearing-down of the womb, and also from piles since her first pregnancy, twenty-two years ago. Finding that there was no disease of the womb to account for her suffering, I inquired if she suffered much pain on defecation, and she replied, "Yes; that when her bowels acted she had great pain; and that as they were generally in a torpid state, she was constantly obliged to take aperients." On examination of the rectum, some old external piles were found, covered by skin, and within the sphincter some internal piles; at the root of one of them a deep fissure was felt, and opposite to it a large polypoid body.

June 16th. External piles cut off, internal piles tied, fissure divided, and polypoid body twisted off at its root. The after-treatment the same as before.

July 16th. Discharged cured.

Case 3.—E. F., aged thirty, married, mother of two children, admitted into the London Surgical Home on the 8th of December, 1859. She has been suffering for the last four years from pain in the womb, sensations of bearing-down, pain in the back, general lassitude, and inability to perform her domestic duties. She had been in an hospital for a month, and was dismissed because she was supposed to have no disease—in fact, that she was shamming illness. On examination, the uterus was found healthy; but on examining the rectum, the same condition was found as in Case No. 1.

Assisted by my colleague, Dr. Hall Davis, I divided the fissure, &c. In a fortnight she was out walking, in a month she left the hospital, and I have since heard of her as being in perfect health, and performing actively her domestic duties.

Case 4.—M. R., aged thirty-five, married, was admitted into the London Surgical Home on March 14th last. For several years she had suffered intense pain about the uterus and rectum; and for the last three years has had caustic applied to the uterus twice a week,

but without the slightest alleviation of her painful sufferings. She had been in the habit of taking three or four grains of opium a day, and her general health seemed completely broken down. On examination, an old fissure of the rectum was easily found, situated behiud two large piles.

March 22nd, 1860. The fissure divided, and the piles removed.

24th. The bowels relieved with less pain than she had had for many years past.

April 4th. Discharged, quite cured.

Remarks.—It will be observed, that when the bowels were relieved, only two days after the operation, she had a marked cessation of pain. This is invariably the case, and is a most cheering fact to both the surgeon and the patient.

Case 5.—H. A., aged forty-three, single, a lady from the country, had been suffering for some years from supposed uterine affection, and had had leeches and caustic applied to the uterus, but still derived no benefit. On examining per vaginam, I could find no disease or anything that required treatment. Menstruation was regular. On inquiring if she felt any pain on defecation, she replied, "Yes, frequently; sometimes acutely." On passing my finger within the sphincter, I found a painful fissure, opposite to it a small polypoid body, and two internal hemorrhoids. I advised that she should stay in town; and, after two or three aperient doses, that she should undergo the usual operation. This was done, and in ten days she was up, feeling quite well, and in a fortnight left town without an ache or pain in uterus or rectum. Two years have elapsed since, and she has continued perfectly well. She has frequently expressed her surprise that she had never been so thoroughly examined before when seeking medical advice.

Case 6.—P. C., a lady from the country, married, without children, had been for many years suffering from what she considered uterine disease, accompanied by painful menstruation, which was supposed to arise from constriction of the os and cervix uteri. Leeches, spongetents, and uterine dilators were from time to time persevered in. Still she kept getting thinner and weaker; and from having been accustomed to ride on horseback, and to take much out-of-doors exercise, she was unable to do either, constantly reclining on her sofa, and being, in fact, a confirmed invalid. On inquiring if she suffered pain on defecation, she replied, "Yes, so much so, that she always dreaded any action of the bowels, and was obliged to take strong aperients; that then the pain extended a long way up the bowels, and left a long, wearying, aching pain, which always made her feel sick and faint for some hours afterwards." On examination, I found the same condition of rectum as in the last case; but the fissure was deep, and evidently of long standing, and the sphincter itself was so firmly constricted as to admit with difficulty the point of my finger. I confidently promised a speedy cure if she submitted to the necessary operation. This

she did on the 2nd ultimo, and on the 4th, her bowels were relieved by castor oil enema, and an enormous quantity of feculent matter removed. On the third day the bowels were also moved after a small dose of castor oil. The patient then, to her intense delight, found that she had none of her old painful suffering. Since then she has steadily and gradually progressed in health and strength. Her face, which had been thick and muddy, is now clear and bright, indicating all the signs of returning health. I could easily increase the number of these cases from my note-book.

Remarks.—Each of these cases clearly illustrates the proposition with which I started, and has a strong practical bearing to every obstetric surgeon. Cases of rectum disease, even without fissure,—such as old and hardened polypus, stricture, prolapsus ani, internal hemorrhoids,—all produce more or less pain to the uterus, and are frequently overlooked because the uterus itself is supposed to be the source of suffering.—*Lancet, July* 14, 1860, *p.* 31.

111.—DEATH AND ALMOST ENTIRE ABSORPTION OF A FŒTUS OF SIX MONTHS' DEVELOPMENT.

By Dr. E. J. Fountain, Davenport, Iowa.

The following history will furnish a hint for a diagnosis in any similar case which may hereafter occur, and throw some light upon those rare and obscure cases of placental absorption, concerning which there has been so much difference of opinion. To me the nature of this case, until its final termination, was a mystery. The patient, whose recent condition has furnished the following report, is a lady of intelligence and refinement, and is the mother of several healthy children. She became pregnant, as she supposed, in the month of June. The symptoms were well marked, and the same as she had formerly experienced under like circumstances. She began to feel motion at about the usual time, and the enlargement of her person continued at the same rate as formerly, until about the 1st of December. At this period she felt slightly indisposed for a number of days, but not sick enough to take her bed or ask for medical aid. From this time her enlargement ceased advancing, but she continued to feel something like motion as before Her health was excellent; better, she thought, than usual. During the winter she fancied she was growing smaller; and this opinion, at first a suspicion only, became confirmed before spring by positive evidence of diminished size of the abdomen. This diminution continued going on, but still she thought she felt motion as formerly, but always light. I was first consulted in reference to the case in the month of April, after the time when she expected to have been confined. Her appearance did not in the least indicate that she was pregnant, but on examination I found a very slight but tolerably well-defined enlargement of the hypogastric

region. An examination per vagina revealed an increased size of the neck of the uterus, and its weight indicated some enlargement of the body itself. From a careful examination I concluded that the uterus could not be quite as large as the head of a child of ordinary size at birth. The lady felt a great anxiety to know the real character of her situation, but I had to confess that I could not determine in my own mind what it might be. She was certain that she felt motion every day, yet here was the uterus too small to contain a living fœtus of nine or even six months, and it was still constantly diminishing in size! Gestation had advanced regularly up to the sixth month, and then appeared gradually to recede, until at the time when she should have been delivered her figure differed but little from its natural appearance, and what remained of the enlargement was constantly diminishing. In the meantime there had been no discharge from the uterus of any kind, and she had suffered none but a slight temporary interruption to her usual good health. The case was a complete puzzle to me; and although various conjectures were made to explain her anomalous situation, they were all quite unsatisfactory to myself. I acknowledged to her that the case appeared a mystery that I could not understand; but as her general health was good, I thought there was no occasion for any active interference. Whatever might be the contents of the uterus, it was evident they were gradually being absorbed without injury to the constitution; and I concluded it would be time enough to interfere when any change in her condition would seem to require it. It is hardly necessary to say, that I referred the motion which she supposed she felt, entirely to her imagination, or to some sensation which she had mistaken for the movements of a living child. A few days after this she began to feel some pain in the back, and her appetite and general health was somewhat impaired. She became nervously anxious about her situation, and was disturbed by unpleasant dreams at night. In the absence of any knowledge of the nature of her condition, her imagination naturally furnished all manner of fancies which were calculated to excite her fears and depress her spirits. She still thought she felt motion, but never at any time had any symptom of labour pains. This was her situation on the 11th of May, eleven months from the commencement of gestation, when I determined to examine the uterus by the aid of the speculum, to explore its contents as far as possible, and endeavour to excite uterine action for the expulsion of whatever it might contain. I first passed into the neck a small silver catheter. This entered about half or three quarters of an inch, when its passage was resisted by some firm but slightly elastic obstacle. I then introduced a small and pointed injecting tube, perforated with a number of small openings at its extremity, and injected a small quantity of aqueous solution of iodine and iodide of potassium. The operation was attended with but very little pain, and I left her with directions to remain quiet and wait for the result. In about an hour, labour pains commenced and

continued regularly until I was called, which was about four hours after the operation. This method of exciting labour pains was entirely experimental on my part, and resulted in the effect desired with far more promptness than I anticipated. On examination, I found a membranous bag filled with a tolerably firm substance presenting in the upper portion of the vagina. As the pains were light, I administered ergot; stronger pains followed in about fifteen minutes, and soon my hand received the expelled mass in unbroken form and without a drop of blood. It was the remains of a fœtus, enveloped in its membranes unbroken, with the placenta attached. The placenta was very hard, evidently much compressed and reduced in substance, and partly rolled upon itself. The contents of the sac appeared almost solid, and of a dark colour. The whole mass was so small that it scarcely filled the hollow of one hand. A very slight flow of blood followed after a little while, and after-pains continued during the night. The patient's convalescence was quite rapid. No lacteal secretion was established, nor had her breasts enlarged in the least. On opening the sac at my office on the following day, I found it contained the greatly compressed and emaciated form of a fœtus. The flesh had been almost entirely absorbed, leaving little more than a skeleton, partially covered with a thin remnant of muscular tissue, and surrounded with a small quantity of an opaque yellowish liquid, of purulent consistence, but quite free from odour. The bones of the head were crowded over and under each other; the body and limbs were doubled up, and the whole compressed, with the placenta, into a small compact ball. Upon unfolding it, the skeleton was found to be entire in substance, and measured ten inches in length, without unlocking the bones of the head.

From the history of this case it is evident that the fœtus had been regularly developed, and reached its normal size and condition until at or near the sixth month of gestation: and that at this period the death of the fœtus had occurred from some unknown cause, and absorption of the contents of the uterus began and continued from this time up to the period of delivery. It is an interesting fact that this dead child should have been carried so long, about five months, without exciting uterine action. How much longer it would have remained had there been no interference, it is, of course, impossible to know; but it is certain that there had been no symptoms of any disposition on the part of the uterus to expel the fœtus. Still more remarkable is the fact that so great an amount of absorption had taken place from the uterine cavity. The size of the patient, and the appearance and condition of the bones of the child, with other particulars stated, furnish evidence that the uterus had contained a living child of usual proportions at six months, and that all but the skeleton of this child, together with the placenta, membranes, &c., had been absorbed to such an extent that the whole mass which was finally expelled was not larger in bulk than a good sized orange. With

this case before us, we may readily believe that the entire absorption of a placenta is a possibility, and that it is more than probable that such an occurrence now and then does take place is established by undoubted evidence of a few cases of this kind, of which fact I am personally assured by Dr. C. R. Gilman, Professor of Obstetrics in the College of Physicians and Surgeons of New York.—*New York Journal of Medicine.—Glasgow Medical Journal, July*, 1860, *p.* 238.

112.—ON DEODORISATION IN OBSTETRIC MEDICINE.

By Dr. THOMAS SKINNER, Liverpool.

On the 3rd of November, 1859, I communicated to the Liverpool Medical Society the value of simple tar-water as a deodorant injection in carcinoma uteri and other affections attended with offensive vaginal discharges. I then stated my belief that the active deodorising ingredient in tar-water, in Messrs. Smith and McDougall's patent disinfectant powder, and in the powders and pastes of MM. Corne and Demeaux, of Paris, was creasote, or its analogue carbolic acid, or some such empyreumatic product. I am still of the same opinion, but further investigation has convinced me that so far as the preparations made from vegetable tar are concerned, besides creasote, the oil of tar acts the most important part as the deodorising agent. I was led to this conclusion by observing that, in preparing tar-water, so long as the tar is capable of communicating its peculiar smell to water, hot or cold, does its deodorising property last. On making this observation I also concluded that the substance giving forth the tarry smell must be the active ingredient; and, as the substance is the oil of tar, I immediately instituted a number of experiments which have proved beyond any doubt, that however powerful creosote may be as an antiseptic and deodorant, it is very much surpassed by oil of tar, if not as an antiseptic, at least as a deodorant.

The oil of tar has been variously denominated oil or spirit of tar or pitch; ol. pini, ol. pini rubrum, ol. petrolei, ol. picis, ol. picis liquidæ, &c. There can be but little doubt that the oil of cade, (ol. cadinum, the huile de cade of the French—which is said to be prepared from the juniperus oxycedrus, or Languedoc juniper) is frequently, at least so far as the article of commerce in this country is concerned, nothing but oil of tar. Pereira says that the oil of tar is "a mixture of various valatile constituents of tar."—(Vol. ii. p. 1205, third edition.) The samples of the commercial oil vary much; I have scarcely seen two alike in colour, consistency, and smell. The best specimen I have seen is in the possession of Mr. Wharrie, chemist and druggist, of 50, Berry-street, in this town.

It was only the other day, while I was hastily glancing at the literature of the subject, that the following facts and opinions presented themselves:—In the 'Pharmaceutical Journal' for 1848–49, Mr.

Griffin states, "Tar is a valuable ingredient in disinfecting mixtures; three pounds of tar will preserve twenty gallons of urine from putrefaction, and enable this valuable manure to be preserved or transported inoffensively." Vol. viii. p. 14.) With this great fact before us, why, may I ask, is not tar turned to some account in deodorising our street, railway, and other urinals? In another article in the same journal, by the late Professor George Wilson, on 'Disinfectants, Antiseptics, and Deodorants,' he says, "The only other substance to which I shall at present refer is pitch-oil, one of the products of the distillation of tar; it is an antiseptic of the most powerful class, and very cheap, and if not used in excess, it is applicable as a deodoriser." (Vol xii. p. 283.)

For the last year or more, while surgeons, both in Paris and in London, have been actively engaged in investigating the subject of deodorisation in their own particular department, it seems strange that obstetricians should have done so little in theirs. It is not because obstetricians meet with fetid effluvia less frequently than pure surgeons; it is not because they are less sensible to their existence from being more used to them; but it arises simply from the fact, that they have never properly directed their attention to the best means of removing them.

In the practice of Obstetric Medicine there is no more crying evil, next to pain, than the universally-complained-of bad odour, inseparably associated with the lying-in chamber, particularly the odour of the bed, bedclothes, and patient. Some are much worse in this respect than others. The chief cause of the offensive effluvia is, without doubt, the lochial discharge. We all know how disagreeable, how heavy and how sickly the odour is; particularly a few days after delivery when it is becoming paler, and is passing into the condition termed by the nurses "green water." If it is so very offensive to us, how much more so must it be to the delicate feelings of the patient, particularly of the more accomplished and refined of the sex; and, if the sickly odour is so disagreeable in private practice, where there is only one individual in a chamber, it must necessarily be worse in an hospital for lying-in women, where several are congregated together. In some of the worst forms of puerperal fever also, the vaginal discharges are sometimes so putrid, and the smell so unbearable, that I have known nurses, and even friends, feel it most irksome to approach the patient, much less to extend to her their kindly offices. These may be looked upon as among the minor advantages to be derived from the practice of deodorisation in obstetric medicine. I would also direct attention to the more important question of the saving of life by deodorisation acting as a prophylactic measure in the prevention of puerperal fever and other *post partum* accidents.

Professor Simpson has shown that nearly 3000 mothers die in child-bed every year in England and Wales alone. He says: "Among these 3000 deaths, a comparatively small proportion only are the

direct result of convulsions, hemorrhage, rupture of the uterus, and the other more immediate or primary complications and accidents connected with parturition. The great majority of these deaths is produced by puerperal fever."

Dr. Simpson here refers only to the deaths arising from puerperal fever, and not to the number of females attacked with the disease.

Granting, that 3000 females die in child-birth annually in England and Wales, that three-fourths of them—which is within the mark—die of puerperal fever alone, then that gives us 2250 deaths annually from fever attending child-birth. Granting, also, that the mortality in the absence of epidemic influence, ranges as high as 1 in 4, in round numbers this will present the fearful amount of at least 9000 women attacked annually with puerperal fever in England and Wales alone. Among the causes of this fearful scourge on puerperal females, there is one which I think has not been sufficiently recognised. I allude to the injurious influence of the *post partum* secretions and discharges of the puerperal female, and more particularly of those who have suffered from a tedious or obstructed labour, and who have been confined within the walls of Lying-in Hospitals.

In reference to the close relation which exists between Lying-in Hospitals and the origin of epidemics of puerperal fever, Professor Churchill, of Dublin, who may be looked upon as one of our best authorities on such a subject, says:—"From a review of the history of the epidemics of puerperal fever, it appears that there is some remarkable connexion between them and the Lying-in Hospitals. I do not mean strictly to assert that the epidemics always originate in, and are kept up by these institutions, but I refer to the fact, that we have no record of any epidemic independent of them in early times." The members of the Academy of Medicine of Paris, in their ever-memorable debate of 1858, have put it beyond a doubt that their Lying-in Hospitals are hot-beds for the generation and propagation of puerperal fever. The very best of them is not exempt from the charge. The Beaujon Model Hospital itself lost one out of every twelve women delivered between the years 1852 and 1856. But, if we contrast the results of private practice in the poorest *arrondissement* in Paris (the twelfth) with the mortality occurring at the Hopital de la Maternité, the bare facts speak for themselves. In the first, where the patients are isolated and attended at their own homes, the mortality is only 1 in every 322 women delivered; whereas, in one of the first hospitals of the city the mortality is so high that one woman dies out of every nineteen delivered within its walls.

Every obstetrician in Paris is convinced of the close connexion between Lying-in Hospitals and the origin of puerperal fever, and the only reliable steps which have been proposed to prevent or ameliorate the evil, have been such as will tend the more effectually to isolate each individual patient. They are all agreed that for want of this the present hospitals are powerless to stop the occurrence of the disease or to reduce the mortality; and that, when the fever make

appearance in the wards, for the sake of humanity the hospitals *should* be closed.

As we can no longer doubt that Lying-in Hospitals and the crowding of parturient women fruitful sources of the origin of puerperal fever, both epidemic and sporadic, let us cursorarily examine whether or not it can arise among cases which are isolated.

Dr. Simpson, than whom no obstetrician has more carefully investigated this important subject, has said, that "in a small ward, or small Hospital, one could almost, as it were, manufacture puerperal fever at will, by crowding a great number of patients together in the same ill-ventilated room." . . . This, no doubt, was true when this experiment was driven, as it sometimes accidentally had been, to an extreme. But it was true, also, in its lesser degree; for Dr. Simpson believed that one great cause of weed, ephemera, and febrile attacks, during puerperal convalescence, was the still too slight attention that was paid to the ventilation of the lying-in chamber. "He had repeatedly, he thought, seen more or less slight febrile action set up in a patient from the curtains being closely drawn around her bed for eight or ten hours during the night, being thus obliged to breathe an air loaded and affected with the morbid animal discharges from her own body." I believe that most accoucheurs will agree with these statements of Dr. Simpson.

Without prosecuting the inquiry further for the present, I think that I am justified in concluding—1. That the decomposition of the organic tissues and fluids thrown off by the skin and vagina of the parturient female is to be looked upon as a fruitful source of puerperal fever and other *post partum* febrile and inflammatory actions. 2. That, if such decomposition can be regarded as an agent in the development of such conditions when patients are isolated, it is evident that it must be a much more active cause in Lying-in Hospitals, where the emanations from the patients are more likely to accumulate, and lead to the generation of a peculiar miasmal condition of the air of a ward. 3. That, as the decomposition of organic matter gives rise to fetid odours, such odours in turn become sufficient evidence of decomposition and its dangers. 4. If the premises be true, then deodorisation of the lochia by an agent which stops decomposition promises fair to become a prophylactic of no mean value in puerperal fever and such like *post partum* accidents.

Any practicable means which, without much trouble and at a cheap rate, will safely and effectually relieve the parturient female of all disagreeable smell about her apartment, bed, or person; which will make the patient more comfortable and less loathsome to herself and friends; and which will at the same time assist in preventing the generation of miasma, or lessen the danger from the poisonous influences associated with putrid or morbid effluvia, must be hailed as a great addition to the Materia Medica of Obstetric Medicine. Such an agent, I think, we have in the oil of tar.—*Med. Times and Gazette Sept.* 8, 1860, *p.* 231.

MISCELLANEOUS SUBJECTS.

113.—ON A NEW MALFORMATION OF THE LOWER LIP, WITH SEVERAL OTHER MALFORMATIONS OCCURING IN THE SAME FAMILY.

By J. JARDINE MURRAY, Esq., F.R.C.S.E., Brighton.

[It is interesting to observe the curious cases of what are improperly called monstrosities in some families. They are proved, however, to be nothing more than an illustration of one of the most interesting laws which regulate our original conformation. As the mason adds one stone after another to his fabric, according to a definite rule, so our own fabrics are built up, portion after portion, according to fixed rules. Sometimes, however, a bit is omitted, and this gives rise to monstrosity—called now-a-days *teratology*, or the doctrine of monsters. We give the following paper, by Mr. Jardine Murray, of Brighton, as one of the best we have lately seen on the subject. Mr. Murray has the merit of having introduced to the notice of physiologists a peculiar and hitherto unrecorded malformation of the lower lip, and he passes in review the various possible explanations of its occurrence. He says:]

Having lately found in four members of one family instances of a singular malformation of the lower lip, which does not seem to have been alluded to by any writer on teratology, I purpose in this paper first, to describe the malformation, and then to consider the various possible explanations which might account for its occurrence. And as in the same family there are other important defects of formation, which, viewed collectively and in their relations to each other, afford interesting illustrations of the fundamental laws of teratology, I shall notice the individual peculiarities in the order in which they came under my observation.

In May, 1859, Dr. Thomson, Physician to the Royal Maternity Hospital of Edinburgh, requested me to see J. H., whose parents were anxious that he should immediately undergo an operation for the cure of hare-lip. The little patient was a well-developed and vigorous child, aged eighteen months. The fissure in the lip was confined to the left side, but extended quite into the nostril; and on this side, also, the anterior part of the hard palate was cleft from want of union between the intermaxillary bone and the left superior maxilla. The intermaxillary bone was twisted on itself, so that the incisor teeth were placed at an angle with the line of their ordinary direction, and partly bridged over the gap in the alveolar ridge. This twisting

caused the portion of the alveolar ridge formed by the intermaxillary bone to overlap that portion formed by the inferior maxilla. The little patient had been brought up at the breast; but, in consequence of the above malformation, he had only been able to apply himself to the left nipple.

Fig. 1.—J. H. (the eighth child), before operation. The condition of the Mouth is well shown in this wood-cut from a careful sketch. The marks on the lower lip indicate the openings of sacculi to be hereafter described.

Fig. 2.—The same, some months after operation. As nearly the entire intermaxillary bone has been preserved, the upper lip is normally prominent.

I believe that if the surgeon can choose his own time in hare-lip, he ought either to operate soon after birth, or wait till the child is two or three years old. But in this case there did not appear to be any reason to fear constitutional disturbance from dentition, which had been somewhat irregular. Both parents were extremely anxious that the operation should not be delayed; for the mother was again pregnant, and both she and her husband firmly believed that the daily contemplation of the infant's hare-lip would have such an effect on the mother as to produce a similar malformation in her yet unborn child. With the assistance of my friends Drs. Pow and Messer, I accordingly performed the operation on the 14th May, 1859. After extracting the incisor teeth, I pared off a small portion of the prominent angle of the alveolar ridge, hemorrhage from the gum being readily controlled by means of a hot wire. I next dissected the lip from the gum for some little distance on each side of the fissure. Then, having put the lip on the stretch by grasping the angle with catch forceps, I removed a crescentic portion from each margin of the fissure, and brought the edges together by means of two needles of

soft steel and two sutures of silver wire. On the fourth day the needles were removed, and the cicatrix protected by a layer of collodion. During the following week, the cheeks were supprted by a strip of adhesive plaster placed across the face.

The operation was entirely successful, and afforded the father so much satisfaction that he brought me another of his children, aged three years, who had been born with webbed fingers, and requested me to perform any operation I might think proper for the removal of the deformity.

On the left hand, the middle and ring fingers were closely united as far as their utmost tips; while, on the right hand, the like fingers were joined together by a web which did not extend much beyond the middle joint. It appeared that the bones and tendons of each finger were perfect, and that two fingers were merely wrapped in a common envelope of skin.

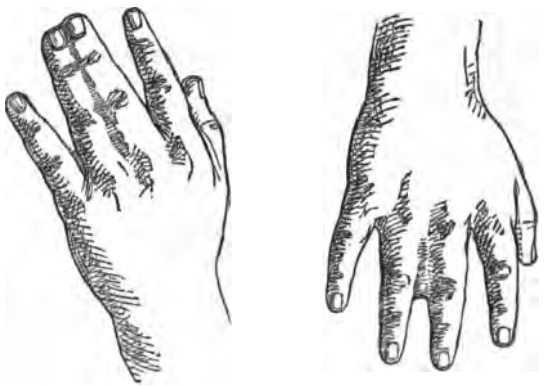


Fig. 3.—Left hand. Webbed fingers. Right hand.
This Boy (the seventh child) was not the subject of any other malformation.

This malformation really consists in absence of fission, and is properly an arrest of development; for in the embryo the rudiments of hands and feet do not at first present any division of the fingers and toes.

I dissected the fingers from each other, and dressed each hand on a Y-shaped splint of gutta percha, the wrist being bandaged to that portion of the splint corresponding with the stem of the letter, and the middle and ring-fingers being respectively placed on the portions representing its arms. Thus, the undesirable re-formation of the web by cicatrization at the angle of divertication of the fingers, which has such a strong tendency to occur in these cases, was effectually pre-

vented. A month afterwards the wounds were entirely healed, the fingers were almost free from stiffness, and the little fellow was using his hands as freely as any child of his age. Neither he nor any of his relatives have webbed toes.

Being now much interested in the peculiarities of this family, I was permitted to examine each of its members in turn; and I am thus enabled to construct the following table, which exhibits in a very striking manner the hereditary nature of physical malformations:—

Father's mother, aged 65 .	Palate very narrow and much arched
Father, aged 41	Double hare-lip. Two sacculi in lower lip.
*Mother, aged 37.	
First child (girl), aged 17 .	Two sacculi in lower lip.
*Second child (girl), aged 15 +.	
Third child (girl), aged 13.+	Double hare-lip, Two sacculi in lower lip.
*Fourth child (girl), aged 11—.	
Fifth child (girl), aged 9+ .	Palate very narrow and much arched.
[Miscarriage.]	
*Sixth child (boy), aged 5.	
Seventh child (boy), aged 3 .	Webbed fingers, both hands.
Eighth child, (boy), aged 1½ .	Hare-lip (see description and figs. above.) Two sacculi in lower lip.

All the individuals alluded to in the above table are free from constitutional disease, and enjoy excellent health.

The father was born with double hare-lip, and was operated on by the late Mr. Liston. None of his brothers or sisters were thus affected. His mother's palate is, however, decidedly narrow and unusually arched in formation; and while she was somewhat eagerly accounting for the existence of hare-lip in her son, by the circumstance that shortly before his birth, she had been frightened by an elephant in Wombwell's menagerie, I was forcibly reminded of Mr. Fergusson's remark that—'Often, while listening to a mother's story about some conjectural cause for her infant's deformity of face, he has thought that a glance at her own features in the looking-glass might have given her a more plausible reason for the condition of her offspring.'

In the contracted formation of the palate, the fifth child exactly resembles her paternal grandmother.

The seventh child, as already mentioned, was born with the middle and ring-finger of each hand closely united by skin.

The eldest child, aged seventeen, is quite womanly in appearance.

* The mother is in every respect well formed, and her second, fourth, and sixth children are also quite normal in development. She does not inherit any tendency to malformations such as are found among her children.

She is in every respect well-formed, save that in the lower lip there are present two pouches or sacculi, which constitute the interesting congenital peculiarity now to be described.

The under lip is full and fleshy. About a quarter of an inch from the external edge of the pink mucous membrane of the prolabium are two crescentic openings, exactly similar in appearance and symmetrical in position, one placed on either side of the mesial line. The horns of the crescent are directed forwards, and a little outwards. A probe inserted into one of these openings passes downwards on the inner side of the lip under a considerable thickness of mucous membrane, to the depth of half-an-inch. In each pouch a split pea might readily be introduced. The pouches do not communicate with each other; they secrete glairy mucus, and their presence does not occasion any inconvenience.

Sacculi in every respect precisely similar to these are present in the under-lips of the father of this family, and of his third and eighth children, all of whom were, however, born with the additional malformation of hare-lip.

By referring to the portraits of the eighth child (Figs. 1 and 2), the indications of the crescentic mouths of the sacculi may be seen on the prolabium of the lower lip. The two following woodcuts are carefully made from photographs of the father and his third daughter.

Fig. 4.—The lower portion of the face of the Father, aged forty-one. He was born with double harelip, and at the age of thirteen was operated on by the late Mr Liston. The prominence of the under-lip is due to the scantiness of the upper-lip, which is drawn tightly over the gap caused by the removal of the intermaxillary bone. The crescentic openings of the sacculi on the prolabium of the under-lip are faithfully represented.

How, then, are we to account for this malformation in the lower lip?

1. The malformation is evidently not consequent on the occurrence of hare-lip; for although the father and the third and eighth children

were born with hare-lip, and also sacculi in the lower lip, the eldest child has the sacculi in the lower lip very large, while her mouth is in every other respect well-formed.

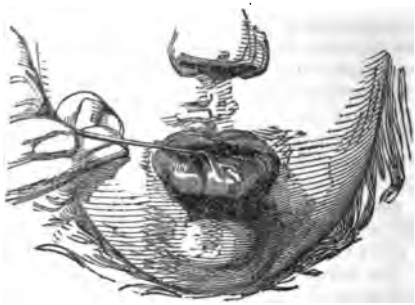


Fig. 5.—The lower portion of the face of the third child, aged thirteen. She was born with double hare-lip, and was operated on in infancy. The intermaxillary bone has been removed, and the upper lip is tightly stretched over the gap in the jaw thus created. The openings of the sacculi are well shown. The mucous membrane round the opening of the sacculus to the right of the mesial line is represented as it appeared when put on the stretch by means of a probe.

2. The sacculi might possibly be ascribed to the erratic development of tooth-sacs in the under lip. But in all so-called "freaks of Nature" in the human subject, there appears to be an approximation to the normal condition of some one of the lower divisions of the vertebrate animal kingdom; and I am not aware that true labial teeth are present even among fishes.

3. The sacculi cannot be ascribed to arrested formation, for in all the four persons in whom they occur there is complete union in the mesial line of the lower lip. And in all mammals, the under lip and lower jaw are developed from two lateral pieces, though the formation is more complex in some reptiles and fishes.

4. Dr. Ormerod has suggested to me that the malformation may possibly be explained by some peculiarity in the development of the frænum. The crescentic form of the mouths of the sacculi, and their perfectly symmetrical character, render the suggestion well worthy of consideration. But, in other respects, the formation and position of the sacculi do not appear to accord with this explanation.

5. Are the sacculi the result of intra-uterine disease of the labial glands? Some considerations favour this view. Surgeons are aware that the labial, like other mucous glands, are frequently the seat of cystic disease. In the 'Guy's Hospital Reports' for 1859, Mr. Birkett has recorded an interesting case of labial cyst, occupying the middle of the prolabium of the lower lip, a little to the left of the mesial line.

The peculiarly wide and crescentic openings of the sacculi, however, and the fact that in all four individuals they are so perfectly uniform in appearance and symmetrical in position, seem opposed to such an explanation.

Congenital malformations are sometimes evidently due to disease of the ovum or fœtus, and probably are often to be attributed to the male and female reproductive elements having been affected prior to the act of conception; but assuredly the most common explanation is to be found in impeded or arrested development of the fœtus, from some remote or unknown cause. Now, if we suppose the sacculi to be consequent on intra-uterine disease of the labial glands, it is to be noted that in each of three members of the family which forms the subject of this paper, we have two malformations—one (hare-lip) due to arrest of development, and the other (sacculi in lower lip) caused by intra-uterine disease. But there is ample proof that intra-uterine disease may co-exist with arrested development of the fœtus.

At my request Professor Goodsir very carefully examined the sacculi; and while he concurred with me in thinking them extremely interesting and remarkable, he confessed himself unable to account for their existence. I have since communicated with Professor Owen, Dr. Simpson, Mr. Paget, Mr. Green, Mr. Quekett, and others whom I thought likely to throw light on the subject. But I believe that a careful record of facts and description of appearances will better fulfil the requirements of science, then a painstaking advocacy of any one possible explanation of this interesting malformation.—*British and Foreign Medico-Chirurgical Review, Oct.* 1860, *p.* 502.

114.—ON THE PRESERVATION OF URINARY DEPOSITS.

By Dr. Lionel Beale, F.R.S., Physician to King's College Hospital.

The different characters of urinary deposits render necessary different plans of preservation. It is, therefore, desirable to consider the nature of the deposit before we attempt to preserve it. Some deposits may be preserved *dry*, others may be mounted in *Canada balsam*. A certain number exhibit their characters very well if preserved in *glycerine*, while many can only be kept in certain *aqueous fluids*.

Of placing the Deposit in the Preservative Fluid.—After the deposit has been allowed to settle in a conical glass, the supernatant fluid is to be poured off; and if it is to be mounted *in fluid*, a quantity of the preservative solution, equal in bulk to the urine and deposit that remain, is to be added. After the deposit has again settled, the fluid is to be poured off and replaced with an equal portion of fresh preservative solution. In this way the deposit is washed clean, and properly impregnated with the preservative fluid.

When preparations are to be preserved in a fluid medium, a small

shallow water-tight cell is to be used. The specimen and its preservative fluid being placed in the cell, the thin glass is applied, and the cover cemented in its place with the aid of Brunswick black or other cement. (*How to work with the Microscope.*) In washing urinary deposits prior to mounting them, it is often necessary to add some compound to the water used for this purpose, in which they are known to be insoluble; and sometimes it is desirable to add some substance to increase the density of the fluid; for which purpose certain salts, syrup or glycerine, may be employed, according to circumstances. Many deposits, although soluble to some extent in pure water, are quite insoluble in a weak acid; others are insoluble in a weak alkali or in certain saline solutions. Again, it is sometimes desirable to separate certain substances in the deposit from others, and this may be effected by special chemical solutions which have the power of acting on the one and not upon the other; or, in cases where one is more dense than the other, by agitating the deposit with water, and, after allowing time for the heavier one to settle, pouring off the lighter one into another vessel, to subside there. From this, it may be collected in the usual way.

If the preparation is to be preserved as a *dry* object, water is to be added in the first place; and a portion of the deposit, which has thus been carefully washed, is to be removed with the aid of a pipette to the glass-slide, and the fluid allowed to evaporate, the whole being covered by a bell-jar, and placed over a dish of strong sulphuric acid. When dry, it is to be protected from dust by a thin glass cover.

The glass cover is easily prevented from pressing upon the preparation by interposing a thin piece of paper or cardboard; or a thin India-rubber ring, which may be easily fixed to the glass-slide and thin glass cover, by a little gum made into a thick paste with whiting, may be used.

If the specimen is to be mounted in *Canada balsam* or turpentine, it is to be dried in the manner just described, warmed slightly, wetted with the balsam, and mounted with the usual precautions. (*How to Work with the Microscope.*)

Refractive Power of the Medium in which Deposits are Mounted.—The appearance of objects in the microscope depends very much upon the medium in which they are immersed; and many structures are so altered in their character by different media, that they would hardly be recognised as the same object. It may be said generally, that the darker the object, and the more dense its structure, the higher should be the refractive power of the medium in which it is mounted—thus the dark coloured uric acid, or the thick spherical crystals of carbonate of lime, and the dumb-bells of oxalate of lime, exhibit their structure to the greatest advantage when mounted in the highly refracting *Canada balsam*, or in *strong syrup* or *glycerine*, while the beautifully transparent octohedra of oxalate of lime would be scarcely visible in these media, and require to be mounted in an aqueous fluid which

possesses a lower degree of refractive power. Many of these objects, when mounted dry, appear quite dark, and scarcely exhibit any structure at all, in consequence of great difference in the refracting power of their substance, and the air by which they are surrounded. From what I have said, it will be evident how important it is to examine the same object in different media—in fact, it is quite impossible to form an idea of the real structure of many specimens, without proceeding in this manner. (*How to Work with the Microscope*, p. 59; and *The Microscope in its Application to Practical Medicine*, second edit., sects. 74, 80, and 90.)

Media in which Urinary Deposits may be preserved.—Urinary deposits may be mounted in *air*, in *turpentine*, *oil*, or *Canada balsam*; in *glycerine*, in *gelatine and glycerine*, in *solution of naphtha and creasote*, in *certain saline solutions*, in *weak spirit*, and in some other aqueous solutions, which will be alluded to. The "*glycerine*" which I use is *Price's patent glycerine*, diluted with water.

The composition of the naphtha and creasote fluid, above referred to, is as follows:—

Solution of Naphtha and Creasote.

Creasote.....	3 drachms.
Wood naphtha	6 ounces.
Distilled water	64 ounces.
Chalk, as much as may be necessary.	

Mix first the naphtha and creasote, then add as much prepared chalk as may be sufficient to form a smooth thick paste; afterwards add, very gradually, a small quantity of the water, which must be well mixed with the other ingredients in a mortar. Add two or three small lumps of camphor, and allow the mixture to stand in a lightly covered vessel for a fortnight or three weeks, with occasional stirring. The almost clear supernatant fluid may then be poured off and filtered, if necessary. It should be kept in well corked or stoppered bottles.

Of keeping the Urinary Deposit for subsequent inquiries.—In cases where it is desirable to retain a certain quantity of the deposit in the preservative solution for subsequent examination, or for the purpose of making more preparations, it should be kept in a small glass tube, with a tight fitting cork, and carefully labelled. Most urinary deposits may be kept for a longer time in this manner, than mounted in thin cells. I propose now to describe briefly the various plans adapted for the preservation of urinary deposits which I have found to succeed best.

First Class Urinary Deposits.

Mucus.—It is very difficult to preserve the character of the so-called "mucus corpuscles", or imperfectly formed epithelial cells, nuclei, and granules, which constitute the slight flocculent deposit met with in healthy urine, and termed "mucus". The naphtha and creasote

solution is best adapted for the purpose, and it is desirable to place the specimen in a cell about the twentieth of an inch in depth.

Epithelium.—The different varieties of epithelium are easily preserved, although, after the lapse of some time, minute oil globules make their appearance in them. They may be kept in naphtha and creasote fluid, to which one-fourth of its bulk of glycerine has been added. It is well to put up specimens of epithelium from the urethra, bladder, ureter, and pelvis of the kidney, removed from the organs of a healthy man who has been killed accidentally. They should be mounted in very thin cells. Specimens of the epithelium from the vagina, which can generally be obtained from the urine of females, should also be preserved.

Vegetable Growths: Fungi.—I have found that fungi may be preserved most satisfactorily in glycerine, for although they appear somewhat more transparent in this fluid than in urine, they preserve their general character better than when immersed in other preservative fluids. It is necessary to add weak glycerine in the first instance, and to increase the strength gradually, otherwise the fungi become collapsed, owing to the great density of the strong solution. A solution composed of equal parts of water and Price's glycerine is sufficiently strong to preserve fungi. I have not been able to preserve specimens of sarcinæ which I have met with on two or three occasions in the urine, probably in consequence of their extreme delicacy. The sarcinæ which are from time to time met with in vomit keep perfectly well, and preserve their recent characters in glycerine.

Spermatozoa are sometimes mounted in the dry way; but although their general form is preserved, their refractive power and transparent appearance are so different from what is observed when they are immersed in urine, that little is gained from such preparations. Spermatozoa keep very well in glycerine, although they appear rather more faint than in an aqueous fluid. They should be examined with the *eighth of an inch object-glass* ($\times$ about 400); but when the eye of the observer has become familiar with the general appearances, they may be readily recognised with a quarter of an inch object-glass ($\times$ about 200).

Casts.—It is not difficult to preserve the character of some varieties of casts. The transparent casts often become covered with numerous minute granules and oil globules, and their character much altered. Granular casts and ephthelial casts often keep very well in the naphtha and creasote solution; but altogether I prefer glycerine, with one-third part of water. Although in many instances the cells they contain are altered, and oil globules appear much more transparent than when in urine, this alteration in character may be easily allowed for. I have some specimens of large waxy casts and epithelial casts which have been kept in the naphtha and creasote solution for upwards of seven years, and still preserve their characters well. The specimens in glycerine, of course, keep admirably. Some casts may also

be preserved in gelatine and glycerine, care being taken that the mixture is not made too hot.

Second Class of Urinary Deposits.

Pus.—Recent specimens of pus may be so readily obtained that it is hardly necessary to attempt to preserve the corpuscles permanently. Their characters alter so much in all the preservative fluids that I have tried, that after they have been put up for some time, it would be difficult to recognise the nature of the perparation.*

Phosphates.—The phosphate of lime, in its amorphous form, in globules, and minute dumb-bells, is easily preserved in weak spirit, naphtha and creasote fluid, or glycerine; but the character of the crystals of the triple, or ammoniaco-magnesian phosphate, could not be retained in this solution. As is well known, this salt is quite insoluble in solutions of ammoniacal salts, and these make the best preservative solutions for it. Crystals of triple phosphate may be kept for any length of time, with their smooth surfaces and their lustre unimpaired, in distilled water, to which a little chloride of ammonia has been added. Phosphate of lime and the stellar form of triple phosphate may be dried carefully, and mounted in Canada balsam; but, of course, the appearance of the crystals is a good deal altered.

Urates.—As the urates are so commonly met with, and as they are generally deposited in the form of granules, there is scarcely any need of mounting them as permanent objects. If desired, however, deposits of this kind may be preserved by adding a little naphtha and creasote fluid to the deposit, which should be left in it for a considerable time before it is put up. Urates which crystallise in small spherical masses, as often occur in the urine of children, and more rarely in irregularly branched processes, may be preserved very well in Canada balsam, or, if preferred, they may be kept in the naphtha and creasote fluid.

Third Class of Urinary Deposits.

Blood Corpuscles become more or less altered in most preservative fluids. I think that those which I have mounted in glycerine (one part glycerine to two parts of water) have undergone the least change.

Uric Acid Crystals are easily preserved as permanent objects. The usual plan is to mount them in Canada balsam. They should be washed, in the first instance, with a little water, to which a few drops of acetic acid have been added. When pretty clean, they may be placed upon a glass slide, with the aid of a pipette, and the greater quantity of the fluid absorbed with a small piece of bibulous paper.

* Dr. Andrew Clarke speaks highly of some fluids, containing bichloride of mercury and arsenious acid, which he has prepared. (*The Microscope in its Application to Practical Medicine*, second edition, p. 237, note.)

After the crystals have been properly arranged on the slide with a needle, they may be dried, by exposure under a bell jar over a dish containing sulphuric acid. When quite dry, they may be moistened with a drop of turpentine, and mounted in Canada balsam. In this operation, a very slight heat should be employed, otherwise the crystals will become cracked in all directions, and more or less opaque. Uric acid crystals, as a general rule, do not keep well in glycerine. In cases where we wish to preserve other substances in the deposit as well as uric acid crystals, the naphtha and creasote fluid will be found to answer very well. I have some preparations mounted in this manner, which were put up six or seven years ago.

Cystine.—Crystals of cystine may be preserved in Canada balsam, the same care being taken in mounting them as mentioned under uric acid, or they may be kept very well in distilled water, or in the naphtha and creasote fluid, to which a little acetic acid has been added.

Oxalate of Lime.—Both the octohedra and dumb-bells may be preserved in glycerine; but the former look very transparent in that fluid. The dumb-bells may also be mounted in Canada balsam, but in this medium the octohedra are almost invisible. When required for polarising, they should be put up in balsam. The dumb-bells keep very well in glycerine.

On Preserving Crystalline Compounds obtained from Urine.—It is exceedingly difficult to preserve many of the crystalline substances obtained from urine in a moist state; but several of them form beautiful microscopic objects when carefully dried. *Urea, nitrate of urea, oxalate of urea, creatine, creatinine, alloxan, hippuric acid, murexid* and many others, may be kept as permanent objects in this manner. In order to prepare them, it is better to cause them to crystallise upon a glass slide; allow the mother liquor to drain off, and immediately, place the slide under a bell-jar over sulphuric acid. Sometimes the crystals may be made in a small evaporating basin, and when drained and dried, a portion of them may be removed to a glass cell, and covered with a piece of thin glass to exclude the dust. Many crystals may be examined and preserved for a considerable time in their own mother liquor, especially when they are very slightly soluble in fluid: but, as a general rule, this plan does not answer very satisfactorily, for, independently of the escape of the fluid from the edges of the cell, a few of the largest crystals grow still larger at the expense of the smaller ones, and the beauty of the specimen is destroyed.—*Brit. Med. Journal, Sept.* 22, 1860, *p.* 736.

115.—*Chloride of Zinc Moulded into Sticks for the purpose of Cauterization.*—Soften gutta-percha in boiling alcohol, and incorporate it with finely-pulverized chloride of lime in a warm porcelain mortar, taking equal parts of each. Then roll rapidly on a porphyry slab, to the diameter of a quill, and divide in fragments, each of which

shall be pointed at one end. Keep these in a wide-mouthed bottle in powdered lime. These sticks remain perfectly hard, are easily handled, cauterize with great regularity, and act as a sponge through which the chloride will slowly exude, becoming liquid by the action of the air and the skin.—*Lancet, July* 21, 1860, *p.* 64.

116.—*Preservation of Bodies for Anatomical Purposes.*—Professor BUDGE has found that bodies may be admirably preserved for a long period of time, whether for anatomical purposes, or for courses of operative surgery, by injecting into the carotid preservative fluid composed of pyroligneous acid and sulphate of zinc, of each from eight to twelve drachms to seven pounds of water. Bodies thus injected have kept well during eight weeks of intense summer heat, without giving rise to any putrefractive smell, the muscles retaining their red colour, and though a little softened, admitting of good dissection. The injection does not prevent the subsequent injection of coloured matters; and the knives used in dissection scarcely suffer at all.—*London Med. Review, July* 1860, *p.* 48.

117.—CASES OF BRONCHOCELE SUCCESSFULLY TREATED BY STRYCHNIA.

By Dr. MURNEY.

The case which first suggested to me the use of strychnia was that of a lady, who stated that up to four years previously her health was rather good, but about that time the catamenia became irregular, and attended with great pain on each periodic return. She assigned as the cause of the thyroid tumour great distress of mind and disappointments. When I saw her the disease was of six months' duration; the right lobe of the thyroid gland was fully as large as a turkey's egg, extending laterally so as to press on the right carotid and jugular vein. A strong pulsation was felt in the tumour, and a purring thrill was communicated to the hand. She had no palpitation of heart. The action of the heart was quiet and regular. She complained of headache, and of a buzzing noise in the right ear. She imagined there was a slight enlargement of the right eye, and that she felt a sensation of fulness in it. The tumour at times impeded her deglutition, and occasionally her breathing also. I attended her from time to time for a year and a half; during that period I gave her everything I believe that has been recommended for this disease. In addition, by my advice she resided at the sea-side, and used sea-bathing; still the tumour continued undiminished. About this time I ceased to attend her. Three months ago, however, she presented herself again. She told me she got relief for some time from her more distressing symptoms, but the tumour never reduced in size. Latterly all the old

symptoms returned in an aggravated degree; the tumour was larger than at first, and the left lobe was now nearly as much enlarged as the right. The difficulty of swallowing was increased; pulse quick and small, ranging from 100 to 120. I prescribed strychnia, in the dose of the sixteenth of a grain three times a-day. At the end of a fortnight the improvement was great, the headache was completely relieved, the bowels became quite regular, and the right lobe of the thyroid had shrunk considerably. She continued to take the strychnia for some time longer, when the gland had almost assumed its natural size. She has now no disagreeable symptom, and her general health is greatly improved. This lady is now married for two years. She is about thirty years of age.

The second case occurred in a housemaid, unmarried, about thirty-five years of age. When I saw her the disease had lasted for twelve months, and the thyroid gland had acquired a very great size, both lobes being equally enlarged. There was a very distinct fremissement all over the tumour. She complained of great palpitation of the heart; pulse 130; great prominence of the eyes. Catamenia irregular, tongue large and flabby, of a whitish pale colour, and covered with a yellow fur in centre; bowels constipated, only acting about once a week. She stated that about eight years ago she was in a situation where she was very hard worked, and in consequence her health began to decline, and the menses disappeared for two years. Somewhere about this time she perceived the swelling in her neck, which soon acquired a very formidable size. Four months after the bronchocele began, palpitation of the heart set in; this was succeeded shortly by the prominence of the eyes. This case baffled and resisted all treatment. Iodine, internally and externally, iron, zinc, digitalis, and biniodide of mercury proved ineffectual. She was subject occasionally for the last year and a half to paroxysms of violent palpitation of the heart. I gave her the strychnia in a fluid form, as she could not swallow pills; I ordered a grain of strychnia, a drachm of dilute sulphuric acid, water seven drachms, to begin with five drops three times a day, and increase a drop each dose up to twenty, three times a day. When she was ten days taking this medicine a great flow of the menstrual discharge came on, lasted a few days, and returned in a week. She persevered in taking the medicine. Soon after the gland began to shrink, and is now so much reduced that one would scarcely imagine, if they were not told so, that it was ever diseased. The prominence of the eyes is less; she is quite aware of this herself. She is now able to close the lids without any effort on her part; this she could not do before the reduction took place in the gland.

The third case is that of a young lady of fifteen years of age. She states that the swelling in her neck began three years ago, shortly after the death of her only brother, to whom she was very much attached, that it gradually increased, but was not of any inconveni-

ence to her more than the deformity that it produced. Both lobes were enlarged, the right more so than the left. There was no remissement in it; the general circulation was quiet, except that the heart became excited on making the least exertion. She had a dry, husky cough, attended with a ringing sound, a pallid look, not the greenish yellow of chlorosis, but the blanched appearance that is seen in cases of hemorrhage. Notwithstanding all this she was not emaciated, but, on the contrary, quite plump. The appetite was rather good, the catamenia regular but scanty in quantity; bowels sluggish, never moved except by medicine. Enlargement of the thyroid gland is very common in anæmic females, independent of any disease of the heart, functional or otherwise. Considering this case to be more or less a case of anæmia, I gave her iron combined with the strychnia in solution, and the result was very satisfactory. In the course of ten days the improvement was great, the bowels became regular, the headache left her, and the languid and prostrate state had completely disappeared, as the gland began to shrink. She is progressing most satisfactorily; the gland is now become quite soft, but it has not the shrunk appearance or contracted feel of the two other cases yet, but I am quite hopeful that it will ultimately become reduced to its natural dimensions.

As to the pathology of this very curious and interesting disease or its cure, I do not intend to do more than merely glance at it; my object is gained when I have brought forward those cases, and detailed their history and treatment; and as strychnia has never been used before (as far as I know) in similar cases, I shall be disposed to consider this treatment of bronchocele as unique unless I hear something to the contrary; and if this treatment proves successful on a more extensive use of it, I think we shall have gained a considerable auxiliary to our therapeutic remedies. A word or two as to the cause of this singular disease; consulting my own experience of the cases that I have seen, I would say by far the greater number, indeed I may say all, had their origin in mental impressions of some kind. Hence I deduce that the most of our cases of bronchocele are the result of impaired innervation or a perverted action of the nervous functions, at least in this country, and not of climate, locality, or the peculiarity of snow water, as is generally alleged to be the cause in other countries. I look on the thyroid gland to be analogous to the spleen, first, in a mechanical view in acting as a diverticulum to the brain, as we know the spleen does to the stomach; and, secondly, in its pathology; enlargement and congestion of the spleen are often the result in certain localities of malaria; very often it is caused by affections of the mind, and the remedy for this affection of the spleen is either tonics or astringents, or both.—*Dublin Hospital Gazette, June* 1, 1860, *p.* 164.

118.—ON VENTILATION: McKINNELL'S SYSTEM.

In arranging his plans for ventilation, McKinnell takes advantage of two simple and well-known facts. If an aperture is made, or a shaft carried through the ceiling of any room, or roof of any building, two currents of air are at once established; and these two currents, instead of jostling each other with human perversity, pass uniformly in definite courses according to fixed laws. The centre of the opening is occupied by an out-draught of the warm exhausted air of the building, while the sides are lined with an inset ting current of the colder and purer atmosphere. One shifting column of air is contained within the other, and the two have a relative movement somewhat like that of the pieces of a telescope. Any one may illustrate this fact of the regular self-inclosed movements of fluids, of differing temperatures and densities, by warming a little water suspending a few floating coloured particles, in a test-tube, over a spirit-lamp. Directly the heat is applied, the coloured matter will indicate the presence of two opposite currents, in the positions and directions just mentioned. McKinnell carries two funnels or shafts, of different sizes and lengths, through the roof of the building he wishes to ventilate. The smaller and longer tube is placed in the centre of the larger one, and thus the two currents of air naturally seeking to pass into and out of the apartment, are separated one from the other; and this concentric arrangement of the two tubes is so devised that the capacity of the central tube shall be equal to the annular space enclosed between its outer circumference and the inner aspect of the external concentric tube. The lower end of the inner tube is moveable, and has an expanding mouthpiece or flange. This trumpet-shaped expansion serves two purposes; if drawn down away from the outer tube, it spreads the incoming cold air over the upper part of the room, and so causes its descent to be more uniform and dispersed; it may also by being raised to the level of the inner opening of the outer tube, be made to act as a valve shutting out altogether the external air, or limiting the currents to the area of the central tube. There is no mystery in this. The contrivance is simplicity itself; as, indeed, are most of the mechanisms adapted to natural laws. As applied to the forms of buildings and to the various compartments of compound structures, it acquires a little appearance of complexity, from the course and partial separation of the tubes, and the addition of appendages, to meet architectural requirements. But the principle remains the same, and the slightest infringement of it of course mars the scheme, and leads to disappointment. It, however, admits of infinite modifications, and can be fitted with equal ease to hats, carriages, and ship-cabins, houses, coal-mines, schools, churches, and dungeons. The system, too, is cheap. At small cost it may be introduced into old houses or parts of houses. Of course, in new buildings, the outlay is proportionately much less. But under any circumstances, no one need be deterred

by fear of expense, especially if contrasted with the money required for rearing a certificated master or mistress. This is a point of the argument which we are almost ashamed to introduce, though with a certain class of minds there is much to be gained by a sort of profit and loss reckoning, in which the tallies are human beings like ourselves. They can be made to act rightly and advantageously in the right direction, by an appeal to a standard with which they are familiar; whereas you would be met with a stolid stare of wonderment if you took your ground for reformation and money-spending upon the natural claim for care and consideration, in the matter of health, of your officials and the little untrained victims of "education." This system meets their case effectually. No matter what the building is, how mean, how close, how ill-contrived, the best air that is outside of it can be got in, and the worst got out, for a mere trifle. The direct outlay would be thought nothing of even by a small, higgling committee; and if universally made, the raw material once manufactured into certificated teachers would last a long while, and the subscriptions to the factory might be in a measure reduced. We actually meet with people of this stamp, and needs must we act upon their intellects and moral sense with what would be caustics and searing-irons to better natures. In conclusion, this scheme of ventilation brought forward by Mr. McKinnell is unquestionably good, easy, and economical, and completely does that which is the great object in keeping buildings healthy and habitable. It rids them of the foul air, and provides for the free inlet of a fresh supply from the outside. —*Med. Times and Gazette, July* 7, 1860, *p.* 11.

119.—FATAL DISEASE PRODUCED BY THE TRICHINA SPIRALIS; ARTIFICIAL PROPAGATION OF THE PARASITE.

Trichina spiralis, know formerly only in a capsulated state, was considered more as a zoological curiosity than as a subject of pathological interest. No symptoms were known to betray its presence; and it was, perhaps, more frequently discovered in the dissecting-room of the students than at the *post-mortems* conducted by the pathologist. Since its discovery by Owen, numerous conjectures have been formed relative to the nature, origin, and propagation of this singular parasite. Herbst believed it to be identical with filaria, Meissner and Davaine regarded it as a larva of trichosoma, and Küchenmeister considered it to represent an undeveloped, juvenile stage of tricocephalus dispar. The latter idea seemed to be confirmed by some experiments of Leuckart, who found trichinæ in the muscles of an animal fed with trichocephali; these experiments could, however, not be considered as conclusive, as the examination of the muscles before the experiment was commenced had been neglected. Herbst was the first to institute feeding experiments with trichinatous muscles. He

found trichinæ in the muscles of the animals experimented upon; but the connective links between the parasites introduced into the stomach and those found in the muscles being deficient, no perfect light was thrown on the subject by these observations. The first experiments of Virchow, instituted last summer, tended materially to supply these deficiencies. In the intestinal canal of an animal fed with trichinatous muscle, he found the villi crowded with psorospermia; and free in the intestinal mucus numerous thread-like worms of the form of trichinæ, of both sexes, the sexual utricle of the male filled with sperm-cells, that of the female densely stocked with ovules. Trichinæ were thus proved to be bisexual. Their non-identity with tricocephali was also placed beyond doubt. The numerous points still requiring elucidation were being reserved for further inquiry, when, in January of the present year, the following case was observed by Professor Zenker, of Dresden, which, in conjunction with the experiments to which it gave the impetus, and for which it supplied the material, not only served to bring about a final settlement of the zoological part of the question, but disclosed the startling and alarming pathological fact, that trichina spiralis, hitherto considered to be an innocent parasite, is in reality the most terrible and dangerous of its kind,—that it can actually kill a healthy adult in a few weeks, under the most distressing symptoms:—

On January 12, 1860, a robust maid-servant, 24 years of age, was admitted into the Dresden Hospital. She had been indisposed since Christmas, and confined to bed since New Year's-day; complaining of depression, lassitude, sleeplessness, loss of appetite, heat, and thirst. These symptoms persisted on her admission; there was considerable pyrexia; the abdomen painful and tympanitic; and although neither splenic tumour nor roseola were present, the case was put down as one of typhoid fever. A remarkable affection of the whole muscular system now rapidly supervened, consisting in extreme painfulness of the extremities, with contractions of knee and elbow-joints, and œdematous swelling, particularly of the legs. The pain was so severe that the patient was continually moaning. Pneumonic symptoms supervened, and death took place on the 27th inst., preceded for twenty-four hours by an apathetic condition. The *post-mortem* examination showed in the internal organs merely an atelectatic condition of the left lung, with numerous small lobular infiltrations, bronchitis and hyperæmia of the mucous lining of the ileum. The muscles, however, which showed a greyish-red colour and a slightly freckled appearance, were found, on microscopic examination, to harbour vast numbers of non-capsulated trichinæ. The parasites were living, some coiled in spirals, others with extended bodies; and all (as Professor Virchow was the first to show, in a fragment of muscle which was forwarded to him for examination) living within the sarcolemma of the primitive fibrils. They showed various stages of development; they were diffused over all the striated muscles of the body, with exception of the

heart, and that in such vast numbers, that, under a small magnifying power, as many as twenty were in the field of vision simultaneously. The muscular substance was otherwise fragile, homogeneous, non-striated, and showed numerous transverse fissures. The intestinal mucus was found to be swarming with mature trichinæ of both sexes; and the remarkable fact was elicited, that female trichinæ are viviparous; the central portion of the bodies being observed to be full of well-developed embryos.

Inquiry being directed to the probable source of the trichinatous infection, it was ascertained that on December 21, four days before the patient was taken ill, two pigs and an ox had been slaughtered in the establishment of her master. Some smoked ham, and sausage prepared from the meat of one of the pigs, were fortunately obtained, and on examination proved to be full of trichinæ. The parasites had a shrunken appearance; otherwise unchanged: reassumed a normal appearance on addition of water, but showed no signs of vitality. It is particularly worthy of remark, that to the naked eye the ham appeared perfectly healthy. It is very likely that the deceased had partaken of some of the raw meat. The butcher of the establishment (butchers notoriously indulge in raw meat) had also been taken seriously ill a short time afterwards, and was confined to his bed for three weeks with severe muscular pains, his whole body being semi-paralytic, &c. This complaint was ascribed to rheumatism at the time, but Professor Zenker correctly surmises that an immigration of trichinæ, not sufficiently extensive to prove fatal, may have been the cause of the attack; and that capsulated trichinæ would very likely be discoverable in his muscle. Professor Virchow immediately commenced a series of feeding experiments with the pieces of human muscle forwarded to him by Professor Zenker.

The following is a brief statement of the results, as published in the last number of Virchow's Archiv.:—

Rabbits fed with trichinæ die in about a month under symptoms of general muscular paralysis.

The trichinæ, which, as long as they reside in muscle, have no perfect sexual organs, become perfectly developed in the ileum. They are found free in the duodenum about six hours after a piece of trichinatous muscle has been introduced into the stomach. In about a month they attain a length of four lines, and during that period not only mature eggs and sperm-cells, but numerous embryos, resembling small filariæ, are developed, which leave the maternal body through the anterior sexual orifice, are found in the mesenteric glands, and rapidly invade the whole muscular system, dwelling within the sarcolemma, and feeding upon the contractile substance of the muscular fibres. They are found in all the striated muscles of the body, with the exception of the heart (Zenker states that he found a few in the heart of a rabbit fed with trichinæ); liver, lungs, kidney, &c., are free. In case the immigration is not sufficiently extensive to cause a fatal

result, the trichinæ become enclosed in a capsule, which consists originally merely in a thickening of the sarcolemma, and this is the only condition in which they were formerly known. The trichina now shows the highest development it is able to attain within the muscle, into which it originally penetrated in an embryonic stage. It still retains its vitality, and quietly waits for an opportunity to find its way back into the intestinal canal, where, as Virchow's observations have shown, the two sexes attain the stage of puberty, and a wonderful productiveness, so pernicious to the individual who is unfortunate enough to harbour such terrible guests, is displayed. The same applies to the non-capsulated trichinæ. The flesh of the rabbit had to the naked eye a perfectly normal appearance. The trichinæ are not near as numerous in this case as they were on former occasions. The intestinal mucus contained mature trichinæ of both sexes.

Professor Zenker made a feeding experiment with a piece of the ham. No trichinæ were found in the rabbit a week after. These experiments will no doubt be repeated, as the time elapesd was far too short to enable a definite conclusion to be formed.—*Edin. Medical Journal, Aug.* 1860, *p.* 173.

120.—*Influence of Animal Food on the Colour of the Skin.*—M. d'Abbadie, the African traveller, writes to M. Quatrefages, that the flesh-eating blacks south of Nubia have a much lighter complexion than those nourished on an exclusively vegetable diet. It is also pointed out that the negroes in Kabylie who eat flesh have a very light complexion, while preserving the crisped hair and all the other characteristics of the negro race.—*Dublin Hospital Gazette, June* 15, 1860, *p.* 178.

121.—*On Periodicity as a Character of Disease.* By Dr. E. J. Tilt. —Believing that the periodical phenomena occurring in the system are to be referred to the ganglionic nerves, and that each menstrual process is analogous to a fit of ague, I was induced, many years ago, to try the best tonor of the vaso-motor nerves—sulphate of quinine, whenever the menstrual period recurred every second or third week instead of at the normal period, the fourth week. I have very seldom found the plan to fail, unless the change in the menstrual type was caused by severe inflammation of the neck of the womb. I have given the sulphate of quinine alone, in doses of from two to three grains every or every other night, immediately after the subsidence of menstruation. The same dose of sulphate of quinine may be combined with two grains of extract of hyoscyamus, or a quarter of a grain of extract of opium, when nervous symptoms predominate, or with sulphate of iron if the patient be anæmic, or with extract of aloes in sufficient quantities to prevent constipation.—*Lancet, Aug.* 11, 1860, *p.* 149.

122.—WHAT IS DISEASE?

By M. CLAUDE BERNARD, Member of the French Institute; Professor of General Physiology at the Faculty of Sciences.

[This celebrated Physiologist asks this interesting question in his 25th lecture published in the 'Medical Times and Gazette,' and endeavours to answer it by showing the effects of some poisons on the system.]

When we examine the lesions which result from the injection of toxic substances, we invariably discover that one peculiar histological element, one given tissue has been invaded in all the parts which compose the animal economy; and the direct consequence of such modifications is a total derangement of the physiological functions which, in the healthy state, devolve upon the injured elements. A general disturbance, therefore, arises from the suppression of their vital properties; and in this respect, the action of poisons is strictly parallel to that of internal diseases. Nor is this all; we are fully aware that the characteristic symptoms which attend the introduction of each particular substance into the system, enable the physician to establish as precise a diagnosis as in the case of any other affection. Are not the effects of arsenic, or those produced by the salts of lead, as easily distinguished from those of other poisons, as typhus fever or pneumonia from all other diseases? Now this is a property which has at all times been viewed as an essential characteristic of well-defined diseases; we give, in fact, this name to a definite succession of morbid phenomena, which arising from a given anatomical modification of tissue, undergoes a regular evolution, and terminates either in death, when the more important phenomena of life have been suspended, or in the patient's recovery when they have resumed their natural course.

But a highly plausible objection might now be raised against us; the effects of poison, it may be said, can be reproduced at all times upon a healthy subject; while morbid agents are entirely beyond our control. Our reply to this argument is, that a variety of diseases may be created by artificial means, especially when a peculiar virus exists; the inoculation of small-pox and of venereal diseases sufficiently prove this; and we have previously shown that mere surgical lesions frequently enable us to create at pleasure in sound animals, a series of well-known disorders.

The astonishing rapidity with which the symptoms of poisoning often succeed the introduction of the toxic agent into the body, ought not to prevent us from assimilating them to the ordinary effects of disease; for the duration of the morbid series entirely depends upon the nature of the poison, and the state of concentration in which it is employed: if prussic acid, when pure, produces instant death, its destructive powers may be reduced by successive dilutions to the very lowest degree; and a large number of other poisons (metallic salts,

for instance) act with comparative slowness, and may be assimilated to chronic affections, when taken in small doses.

The absence of all visible lesions after death, which not unfrequently occurs in cases where poison has been administered: and the possibility of an entire recovery, when the noxious substance has been completely expelled, may be adduced as additional proofs in favour of our views; and lastly, as a further resemblance to certain diseases, an incubation of a certain extent often takes place. The deleterious agent does not always exhibit at once its destructive properties, but remains during a certain lapse of time in a latent state (so to speak) within the economy. In short, the phenomena of life, whether viewed in connexion with physiology, pathology, or toxicology, obey the same natural laws, and ought not to be studied apart from each other. But in the case of poisons, the observer is placed in the most favourable conditions that can possibly be imagined: he administers definite substances in given quantities; he notes the effects produced from the very commencement of the consecutive disorder,—a condition which can never be realised in clinical practice; and lastly, he selects according to his convenience, the age, size, constitution, and species of the animal on which the experiment is performed: he therefore enjoys every possible security for the perfect success of the operation.—*Med. Times and Gazette, Sept.* 8, 1860, *p.* 225.

123.—ON THE GENERAL EFFECTS OF MEDICINAL SUBSTANCES.

By M. Claude Bernard.

[In order to come to a rational explanation of the effects produced by foreign substances when introduced into the system, the first thing necessary is to regard the phenomena observed as ordinary physiological effects.]

In examining the various tissues which enter into the composition of the living frame, it is easily ascertained that peculiar and distinct properties are separately enjoyed by each of them; and that, in this manner, they are enabled to fulfil the various functions which devolve upon them. At the same time we discover that certain general systems exist, which by their distribution are brought into constant communication with all the various elementary tissues of the body; we have already named the vascular and nervous systems. The blood is brought through the capillaries to every part of the body, and its presence being indispensable to the physiological existence of all living tissues, they rapidly perish when accidentally deprived of this all-important and vivifying fluid. The blood, therefore, must be looked upon as the medium in which all our tissues have their being; it brings them in contact with an immense number of substances, derived from various sources, some of them drawn from the atmos-

phere through the process of respiration, others contained in the animal's food, and assimilated by the digestive organs; others again introduced into the economy through various absorbent surfaces, and all of them equally necessary to the continuance of the physiological conditions of life.

Such, however, are not the only bodies which the circulating fluid brings into contact with the primitive elements of which our organs are composed; the blood is, at the same time, the vehicle through which medicinal agents, intended to modify their properties, are conveyed into living tissues, and penetrate into their substance. We must, in consequence, consider medicines as physiological agents of an unusual nature; introduced into the system through the same channel as the bodies with which it is naturally supplied, they reveal their presence exactly in the same manner, viz. by giving birth to a series of phenomena, peculiar to each substance, and widely different from those which exist in the physiological state. But another question now presents itself; *how* are these foreign substances enabled to act upon our tissues? Not through their physico-chemical properties, as has been sufficiently proved in our last lecture; a *physiological* property must therefore be had recourse to, as in the case of all the powers which support life. When oxygen, for instance, is brought into contact with elementary, nervous, or muscular fibres, it quickens their activity, and keeps alive all their properties within them; and, when deprived of so powerful a stimulus, both nervous and muscular fibres quickly die. Similar actions are, no doubt, in their intimate essence, of a physical or chemical nature; but in the present state of our knowledge, the observer can only point to a physiological agency.

Histological elements, as we have already shown, are in every case endowed with special properties; and the wider is the difference which separates one tissue from another, the higher is the animal's rank in the scale; in the nobler species peculiar organs are adapted to each individual function; and in no other animal is their number so great as in man. We are, therefore, justified in supposing that the primitive muscular fibre, for instance, is widely different, in every respect, from the nervous filament and the glandular cell; and, in consequence, that agents entirely powerless as regards all other tissues, may be found to exercise, over the muscles, an almost unbounded influence.

It is therefore our opinion that medicines, as well as poisons, exert their power exclusively upon certain histological elements, even in cases in which they appear to produce a general perturbation of the whole system. Thus, for instance, we find that strychnia localises its action entirely upon the sensitive nerves; and that wherever the torrent of the circulation conveys it, all the properties of the recurrent nervous fibres are destroyed: so that even before reaching the spinal cord it has already paralysed the extremities of the nerves which revert towards their central axis. On the other hand, woorara con-

centrates upon the motor nerves alone its noxious influence, and wherever it meets them, paralyses at once their properties; and in this manner, without doing the slightest injury to glands, muscles, vessels, or other tissues, it arrests at one blow the most indispensable functions of life. But the action of poisons introduced into the blood is not invariably directed towards the nervous system. All the other tissues, and even the blood itself, have their own peculiar poisons, capable of modifying their vital properties. Both digitalis, and the juice of the upas antiar, enjoy the power of destroying muscular contractility throughout the entire system. We find, however, a difference in the intensity with which these poisons act on the various orders of muscles; a difference with which the stimulating properties of electricity, which act so powerfully upon certain systems of muscles, and are comparatively so feeble as regards others, has already rendered us familiar. But the nervous system, after the introduction of these poisons into the economy, remains wholly unimpaired; and through the suspended action of the heart alone is it in their power to produce death. A substance of a very different kind, the oxyde of carbon, neither attacks muscular nor nervous fibres. It acts exclusively on the blood-globules, which of course are to be viewed in the light of a living tissue, as well as other histological elements. Introduced into the lungs, the oxyde of carbon fixes upon the globules which circulate in the pulmonary vessels; and such is its affinity for the blood-cells, that a perfectly stable chemical compound appears to be produced, which the presence of oxygen is no longer able to destroy. In this manner is the important function which devolves upon the globules abolished at once. How, in fact, would it be possible any longer for them to imbibe carbonic acid, and exchange it for oxygen in the capillaries of the lungs? The purposes of respiration being in this manner eluded, the blood-globules circulate in the vessels side by side with carbonic acid gas held in solution in the blood, without contributing in the slightest degree to the natural process of elimination which takes place in the lungs; and the animal at last dies in a peculiar state of asphyxia, the blood still retaining as before its ruddy arterial colour. Here, then, we find an influence of the most extensive and general kind, exerted over the whole economy through exclusively local means; a peculiar histological element having been modified, all the other tissues are deprived of an indispensable stimulus,—a fact which amply illustrates the mechanism through which the effects of poison ultimately pervade the whole frame: the toxic substance, circulating with the blood, fixes upon its favourite tissue wherever it exists. To adduce one more instance of this: the action of arsenic entirely resembles that of the substance we have just mentioned. It fixes also upon the blood-globules, but does not entirely suppress their action; it only tends to diminish the activity of the perpetual exchange carried on by their means between the oxygen and carbonic acid; a state of things which, when confined within proper limits, is

not wholly incompatible with health, and, under such circumstances, produces, as a natural result, a certain degree of obesity.—(Schmidt.)

The action of medicinal substances must not be viewed in a different light; it resembles, in every respect, that of poisons. Nerves, muscles, blood-globules, and other tissues, enjoy peculiar vital properties, the intensity of which is modified through medical interference, sometimes for the purposes of increasing, sometimes for that of lowering, their activity; and the action of foreign bodies, whether medicinal or poisonous, can be rendered general only through the nervous or vascular system. But, in this respect, a wide distinction arises between sensitive and motor fibres, which experiments on living animals have placed in a strong light. Let woorara be injected into an animal's veins, or simply introduced under the skin after the vessels of one of its extremities have been tied, the motor nerves are paralysed throughout the system, and it becomes impossible to produce muscular contraction by acting upon them; but the nerves of that single limb, the vessels of which have been tied, alone retain their physiological power, and provoke contraction when galvanized or otherwise excited. It is therefore clear that motor nerves give merely birth to local phenomena, and are incapable of acting reciprocally upon each other through the central axis, in which they all unite. Far different is the mode of action of the sensitive fibres: let strychnia, or any other poison which holds them under its influence, be introduced into the circulating fluid, after tying, as before, the principal vessels of one limb, and it will be found that the operation has been performed in vain, convulsions being produced in that very limb, through mere nervous agency, as well as on other points. Supposing *all* the posterior roots which arise from the spinal cord to have been divided, no convulsions, of course, would be produced; but let a single root be left untouched, and through this single channel the necessary impulsion will be given: the blood which circulates throughout the system, and conveys the toxic substance into every part, brings it in contact with the extremities of that sensitive root, which alone remains uninjured, and still communicates with the spinal cord; and general effects are at once produced. How different, therefore, do we find the part played by sensitive and motor nerves to bed!

It would therefore appear that the various substances which the physician employs exert their influence, not upon organs, but upon tissues; not upon the apparatus itself, taken as a whole, but upon *some* of the histological elements of which it is composed. It would be an entirely mistaken notion to suppose that digitalis paralyses the heart; not upon the heart itself, but upon the muscular fibres which enters into its structure, is the action of that body directed, the nerves, vessels, &c. remaining, in the meantime, perfectly sound.

There is, besides, no room whatever for astonishment, in the extraordinary energy which poisons display, when administered to the higher animals, while the lower classes remain infinitely less sensible

to their action. A bird is struck dead, as if by lightning, by a dose of digitaline, which slowly destroys a mammiferous animal of an equal size, and is not sufficiently strong to kill a reptile. The fact is averred with regard to other toxic substances: histological elements are submitted, in fact, to the influence of external agents, in direct proportion to their vital energy.

But not only do the tissues of various animals differ with respect to the degree of sensibility which they exhibit under the action of foreign bodies; the proposition is equally true, when applied to homologous tissues, in the very same animal. Thus digitaline, long before it acts upon the contractile powers of other muscles, arrests the motions of the heart; a result which may, no doubt, be attributed to the ceaseless activity of the cardiac fibres: being, therefore, of all muscles the most energetic in the performance of their functions, they are naturally found to be, at the same time, those which experience long before the rest, the noxious influence of poisons. We are aware, besides, that a striking inequality is observed in the results obtained by stimulating various systems of muscles through different means: and that muscular fibres which act in obedience to the will, are infinitely more sensible to the galvanic stimulus than those which it is not in our power to contract at will.

The general effects obtained by the use of medicinal agents, are to be explained by the modifications which they produce in the primitive elements of which our tissues are composed.—*Medical Times and Gazette, May* 26, 1860, *p.* 515.

124.—ON FERMENTED AND AERATED BREAD, AND THEIR COMPARATIVE DIETETIC VALUE.

By Dr. J. Dauglish.

[Since the introduction of the new process of preparing bread, by imparting to it a most perfect vesicular structure by artificial means, many have doubted whether the changes which the constituents of the flour, especially the starch, undergo during the ordinary fermentive process, are not essential to healthy digestion.]

In order to dispose of the assertion that starch requires to be prepared by the fermentive changes to render it fit for human food, it is but necessary to remark, that the proportion which the inhabitants of the earth, who thus prepare their starchy food, bear to those who do not, is quite insignificant. Indeed it would appear that the practice of fermenting the flour or meal of the cereal grains is followed chiefly by those nations who use a mixed animal and vegetable diet, while those who are wholly fed on the products of the vegetable kingdom reject the process of fermentation entirely. Thus, the millions of India and China, who feed chiefly on rice, take it for the most part simply boiled; and that large portion of the human race who feed on

maize, prepare it in many ways, but they never ferment it. The same is true with the potato-eater of Ireland, and with the oatmeal-eater of Scotland. Nor do we find that even wheat is always subjected to fermentation; but the peculiar physical properties of this grain appear to have tasked man's ingenuity more than any other, to devise methods of preparing from it food which shall be both palatable and digestible. In the less civilized states, a favourite mode of dressing wheat grain has been, by first roasting and then grinding it. On the borders of the Mediterranean it is prepared in the form of maccaroni and vermicelli, while in the East it is made into hard thin cakes for the more delicate, and for the hardworking and robust into thicker and more dense masses of baked flour and water. Even in our own nurseries wheaten flour is baked before it is prepared with milk for infants' food. The necessity of subjecting wheaten grain to these manipulations arises from its richness in gluten, and from the peculiar properties of that gluten. If a few wheaten grains are taken whole and thoroughly masticated, the starchy portions will be easily separated, mixed with the saliva and swallowed, whilst nearly the whole of the gluten will remain in the mouth in the form of a tough tenacious pellet, on which scarcely any impression can be made. A similar state of things will follow the mastication of flour. In this condition, the gluten is extremely indigestible, since it cannot be penetrated by the digestive solvents, and they can only act upon its small external surface; hence the necessity to prepare food from wheat in such a manner as shall counteract this tendency to cohere and form tenacious masses. This is the object of baking the grain and the flour as before mentioned, of making it into maccaroni, and of raising it into soft spongy bread; by which latter means the gluten assumes a form somewhat analogous to the texture of the lungs, so that an enormous surface is secured for the action of the digestive juices: and this I believe is the sole object to be sought in the preparation of bread from wheaten flour.

Wheat is said to be the type of adult human food. It supplies, in just proportions, every element essential to the perfect nutrition of the human organism. And yet in practice we find that the food which we prepare from it, and furnish to the inhabitants of our large towns and cities, is quite incapable alone of sustaining the health and strength of any individual. This is the more remarkable, since in Scotland we find that the food prepared from the oat, a grain possessing the same elements of nutrition as wheat, though in a coarser form,—furnishes almost the exclusive diet of a very large number of the hardiest and finest portion of the population.

In the large towns of France wheaten bread certainly forms a very large proportion of the diet of the labouring classes, but not so large as oatmeal does in Scotland. And yet it has been remarked by contractors for public works on the Continent, that the chief reason why the Englishman is capable of accomplishing double the work of a

Frenchman is, that the one consumes a very large proportion of meat, while the diet of the other is chiefly bread. In Scotland, however, the labouring man is capable of sustaining immense fatigue upon the nourishment afforded by oatmeal porridge.

The deficiency of wheaten bread in affording the nourishment due to the constituents of the grain, is to be attributed solely to the mode of preparing the flour, and the process followed for making that flour into porous bread.

The great object sought after both by the miller and the baker, is the production of a white and a light loaf. Experience has taught the miller that the flour which makes the whitest loaf is obtained from the centre of the grain; but that the flour which is the most economical, and contains the largest proportion of sound gluten, is that which is obtained from the external portions of the grain. But while he endeavours to secure both these portions for his flour, he takes the greatest care to avoid as much as possible, by fine dressing, &c., the mixture with them of any part of the true external coat which forms the bran, knowing that it will cause a most serious deficiency in the colour of the bread after fermentation.

It is generally supposed that the dark colour of brown bread—that is of bread made from whole wheaten meal, is attributable to the coloured particles of the husk or outer covering of the grain. But such is not really the case. The coloured particles of the bran are of themselves only capable of imparting a somewhat orange colour to bread, which is shown to be the fact when whole wheaten meal is made into bread by a process where no fermentation or any chemical changes whatever are allowed to take place. Some few years since a process was invented in America for removing the outer seed coat of the wheat grain without injuring the grain itself, by which it was proposed to save that highly nutritious portion which is torn away, adhering to the bran in the ordinary process of grinding, and lost to human consumption. The invention was brought under the notice of the French emperor, who caused some experiments to be made in one of the government bakeries to test its value. The experiments were perfectly satisfactory so far as the making of an extra quantity of white flour was concerned, but when this flour was subjected to the ordinary process of fermentation and made into bread, much to the astonishment of the parties conducting the experiments, and of the inventor himself, the bread was brown instead of white. The consequence, of course, has been that the invention has never been brought into practical operation.

It has been estimated that as much as ten or twelve per cent. of nutritous matter is separated adhering to the bran, which is torn away in the process of grinding, and until very lately this matter has been considered by chemists to be gluten. It has, however, been shown by M. Mège Mouries to be chiefly a vegetable ferment or metamorphic nitrogenous body, which he has named Cerealin, and another body, named vegetable Caseine.

Cerealin is soluble in water, and insoluble in alcohol. It may be obtained by washing bran, as procured from the miller, with cold water, in which it dissolves, and it may be precipitated from the aqueous solution by means of alcohol; but like pepsine, when thus precipated it loses its activity as a solvent or ferment.

In its native state or in aqueous solution it acts as the most energetic ferment on starch, dextrine, and glucose, producing the lactic and even the butyric changes, but not the alcoholic.

It acts remarkably on gluten, especially when in presence of starch, dextrine, or glucose. The gluten is slightly decomposed at first, giving ammonia, a brown matter, and another production which causes the lactic acid change to take place in the starch and glucose. The lactic acid thus produced immediately combines its activity with that of the cerealin and the gluten is rapidly reduced to solution.

The activity of the cerealin is destroyed at a temperature of 140° Fah., according to M. Mouries, but my own experiments show that it is simply suspended even by the heat required to cook bread thoroughly; thus bread made without fermentation, of whole wheaten flour, or of flour in which there is a large proportion of cerealin, will, if kept at a temperature of about 75° to 85° Fah., pass rapidly into a state of solution, if the smallest exciting cause be present, such as ptyaline or pepsin, or even that very small amount of organic matter which is found in impure water—while the same material, when it has been subjected to the alcoholic fermentation, will not be affected in a like manner.

The activity of cerealin is very easily destroyed by most acids, also by the presence of alum; and while it is the most active agent known in producing the earlier changes in the constituents of the flour, it cannot produce the alcoholic, but as soon as the alcoholic is superinduced the cerealin becomes neutralised and ceases to act any longer as a solvent. M. Mouries, taking advantage of this effect of the alcoholic fermentation. has adopted a process by which he is enabled to separate from the bran all the cerealin and caseine which are attached to it. He subjects the bran to active alcoholic fermentation. which neutralises the activity of the cerealin, and at the same time separates the nutritious matter; and then having strained this through a fine sieve, he adds it to the white flour in the preparation of white bread, by which an economy of ten per cent. is effected, and the colour of the bread is not injured.

The peculiar action of cerealin as a special digestive solvent of the constituents of the flour—gluten, and starch—has been practically tested by Mr. Stephen Darby, of Leadenhall-street, in a series of careful experiments. He found that when two grains of dry cerealin were added to 500 grains of white flour, and the whole digested in half-an-ounce of water at a temperature of 90° for several hours, ten per cent. more of the gluten, and about five per cent. more of the starch, were dissolved than when the same quantity of flour was sub-

jected to digestion without the addition of the cerealin, but in which of course there was the small amount of cerealin that is present in all flours. The action of cerealin upon the gluten of wheat is precisely similar to that of pepsin on the fibrine of meat. Pepsine acting alone on fibrine dissolves it, but very slowly, but if lactic acid be added solution takes place very rapidly. In like manner the starch present with the gluten of wheat is acted upon by the cerealin, and produces the necessary lactic acid to assist in the solution of the gluten by cerealin.

With the knowledge thus obtained of the properties of this substance cerealin, it is not difficult to understand why the administration of bran-tea with the food of badly-nourished children, produces the remarkable results attributed to it by men both experienced and eminent in the medical profession; and why, also, bread made from whole wheaten meal, which contains all the cerealin of the grain, should prove so beneficial in some forms of mal-assimilation, nonwithstanding the presence of the peculiarly indigestible and irritating substance forming the outer covering of the grain.

It will be seen that in all the methods of bread-making hitherto adopted, the peculiar solvent properties of this body, cerealin has been sought to be neutralised simply because it destroys the white colour of the bread during the earlier stages of panary fermentation. It is by thus destroying the activity of the special digestive ferment which nature has supplied for the due assimilation by the economy of the constituents of the wheaten grain, that wheaten bread is rendered incapable of affording that sustenance to the labouring man which the Scotchman obtains from his oatmeal porridge. Although the new bread has been as yet but little more than experimentally introduced to public consumption, I have already received from members of my own profession, who have recommended it in their practice, as well as from non-professional persons, accounts of the really astonishing results that have followed its use in cases of deranged digestion and assimilation. Private gentlemen have sought interviews with me to record the history of their recovery to health, after years of suffering and misery, by the simple use of the bread as a diet. Children that have been liable to convulsive attacks from an irritable condition of the alimentary canal and nervous system, have been perfectly free from them immediately the new bread was substituted for fermented bread. And cases are now numerous that have been communicated to me by Medical men of position, in which certain distressing forms of dyspepsia, which had remained intractable under every kind of treatment, have yielded as if by magic almost immediately after adopting the use of the aërated bread.

The delicate flavour of the new bread renders it peculiarly grateful to the stomachs of invalids and children, as well as of those whose tastes have not become vitiated by the habitual use of baker's bread, which is slightly sour, and tastes of yeast. The new bread was sup-

plied to two wards in Guy's Hospital in place of the ordinary bread (which is of a very fine quality, made on the premises,) for two months, and in no case were there any pieces left in the wards unconsumed, while of the fermented bread large quantities of scraps are collected daily, for the consumption of which the appetites of the patients have been deficient.

That persons who have been long used to the strong yeasty-flavoured bakers' bread should consider the new bread tasteless at first is not to be wondered at, since the delicate sense of taste is of all other senses the most easily lost by rough usage. Hence the argument put forth in defence of adulteration by some London tradesmen, especially the beer sellers, that the public will not buy the pure article, as it is wanting in the flavour to which they have been accustomed; and hence, also, the dislike of the Viennese of the fresh oysters supplied to them when the railway was completed, as they deemed them insipid, after the habitual use of oysters slightly decomposed, with which they had been supplied when it required a lengthened period to transport them from the sea.

I am disposed to attribute the beneficial effects of the new bread to two causes. The one to the *absence* of the prejudicial matters imparted to ordinary bread by the process of fermentation, and the other to the *presence* in the bread, unchanged, of that most essential agent of digestion and assimilation, cerealin.

I believe the prejudicial matters imparted to bread by fermentation to be chiefly two—acetic acid and the yeast-plant. The first is produced in large quantities, especially in hot weather, by the oxydation, by atmospheric contact, of the alcohol produced. The second is added when the baker forms his spunge, and is also rapidly propagated during the alcoholic fermentation, and cannot of course be afterwards separated from the other materials in the manner that the yeast and the other *débris* of fermentation separate themselves from wine and beer by precipitation in the process of fining. Nor is the life of the yeast-plant generally destroyed in baking, because it requires to be retained at the boiling point for some time before it is thoroughly destroyed; and bread is generally withdrawn from the oven, for economical reasons, even before the centre of the loaf has reached the temperature of 212°. It is not difficult to understand how the most painful and distressing symptoms and derangements may follow the use of bread in which the yeast-plant is not thoroughly destroyed previous to ingestion, in those cases of impaired function in which the peculiar antiseptic influence of the stomachal secretions is deficient, and is incapable of preventing the development of the yeast-plant in the stomach, and the setting up of the alcoholic fermentation to derange the whole process of digestion and assimilation.

The presence of cerealin in bread is as beneficial as that of acetic acid and the yeast-plant is prejudicial. Digestion, or the reduction of food, is evidently essentially dependent on the action of a class of

substances which chemists, for want of a better term, have called ferments,—to these substances belong pepsine, ptyaline, emulsion, diastase, and cerealin; these are evidently types of a very numerous class, which act by producing those molecular changes in organic substances in which digestion consists; and since the purpose of digestion or solution is to prepare from heterogeneous substances taken as food a chyle, which shall not only when absorbed present all the elements of healthy blood, but shall previous to absorption, possess the properties which will constitute it the proper stimulus to the functional activity of the lacteals, it would appear to be necessary that each distinct substance taken as food should be furnished, not with its simple chemical solvent, but with that peculiar form of solvent or ferment which alone can carry it through those molecular changes which shall terminate in the production of healthy chyle. Hence, we should infer that a substance is digestible or indigestible just in proportion to the provision that is made for its reduction to the standard of healthy chyle, and that substances which have hitherto been incapable of affording any nutrition whatever, may at some future day be rendered highly nutritious, simply by adding to them suitable ferments, artificially obtained or otherwise, that shall secure their passing through the proper molecular changes. Indeed, I think this subject opens up to us that very wide field of inquiry, as to whether the cause and prevention of disease, and the beneficial administration of remedies may not, for the most part, if not entirely, be dependent on the action of substances analogous to such bodies as ptyaline, pepsine, cerealin, &c., acting in concord with, or retarding and opposing the vital functions of tissues; and that by more profound inquiry in this field of research, the physiologist and the pathologist may not at a future day lay the foundation of true scientific Medicine.—*Med. Times and Gaz. May* 12, 1860, *p.* 468.

125.—ON THE NATURE OF DEATH FROM CHLOROFORM.

By Dr. CHARLES KIDD.

There have been about 125 deaths from anæsthetics in Europe up to the present date. A very large number, the only list of any such cases on the Continent, is one by M. Scoutetten, an Army Surgeon, who gives 40 deaths, but unfortunately does not specify the nature of the operation for which the anæsthetic was administered. Dr. Snow gives 50 deaths, and I have since myself made out 35 deaths in addition. All agree as to the vast preponderance, however, of deaths in male patients rather than in females; it also appears that while there have been in reality no deaths as the result of large amputations, resections, ovariotomy, &c., fully two-thirds of the deaths have been from operations on sphincters or tendinous sheaths, strabismus, tooth-drawing, &c.

Nature of Operation causing Death.	Snow.	Kidd.
17 Deaths during operations on fistulæ, hemorrhoids, applications of caustics, &c.	13	4
14 Deaths in removal of toe-nail, dead finger, and such operations on phalanges	9	5
10 Dentists' cases of tooth-drawing, &c.	6	4
11 Deaths from removal of tumours	7	4
Resections, large amputations, midwifery, ovariotomy, ligatures of large arteries, &c.	—	—
9 Deaths in minor amputations (but 6 occuring before the operation commenced)	4	5
6 Deaths in strabismus, operations on eye, &c., cysts of eyelids, &c.	2	4
9 Deaths in operations on testis, sounding, &c. (2 included doubtful in 218 lithotomy cases)	6	3
5 Deaths from reduction of dislocations	2	3
1 Death from strangulated hernia	1	—
3 Deaths from delirium tremens	—	3
	50	35

Scoutetten's cases (not specified) 40

This table explains itself; the general result is very curious, and I think very convincing, that in the hundreds of thousands of operations where chloroform has now been used, it has proved in the vast majority to be perfectly safe, and that it is safer in large than in small operations. Why it is so, I may not now stop to enquire. Again, of sixteen deaths under chloroform, where I was myself able to trace out the probable cause of the fatal issue, I found three where some remote disease of the heart might be suspected; but in thirteen there had been probable, and in some very serious derangement of the nervous system—viz., three deaths from delirium tremens and chloroform combined, two where chorea and meningitis were present, two from cysts in the brain, and four from other lesions of cerebral tissue. It seems too evident, I think, that if syncope or "fit" should occur in any such case, resuscitation is rendered difficult, as we have an imperfect brain and spinal apparatus to work with. Disease of the heart is a very rare cause of death from chloroform; the facts on which this hypothesis was built up are now shown to be all erroneous.

There is a sad want of uniformity in the mode of making post-mortem examinations in our London hospitals. In two necropsies in the Borough hospitals, and one at the Royal Ophthalmic, Moorfields, I was present, and anxious to see the state of the heart and pericardium before the latter was opened; in two I was successful, but

in the third the head was opened first, and a large drainage of fluid blood took place from the jugular veins, sinuses, &c., which left the heart flaccid. Even in one of the other cases, where in good routine fashion the hospital porter opened roughly all the three cavities at once, and left them all open for an hour, awaiting some "crowner's quest" or magisterial inquisition, it was not less difficult to come to any steady or useful decision on the case. I observed, however, while waiting this hour, that the right cavities of the heart were very much distended with fluid blood; the pericardial contents and pericardium bulging very prominently, "filled to bursting almost," till the examination of the lungs commenced, when the section of the large vessels of course emptied the heart; the heart being then examined was marked "perfectly healthy—and empty." It seems to be not a bad mode of proceeding to open all the cavities together, but our great want in the hospitals is some uniformity; and in a very painful degree some uniformity in the examination of dead bodies after accidents from chloroform. Some other remarkable facts have been discovered during the course of these investigations, as to death from chloroform, which it is of interest to note. First, that a very large number of the twenty-five deaths from the use of anæsthetics after operations have been deaths from the effects of the slow administration of ether, or ether and chloroform mixed, but not from pure chloroform. I published nineteen deaths from ether two years ago, yet it is still suggested in America that there have been no deaths from this agent, Scoutetten gives five; and since then there have been at least four or five more from ether or amylene; so that, on the whole, there appears

	Snow.	Scoutetten.	Kidd.	Total.
In 121 deaths, the relative dangers of the chloroform "Stages" were:—				
Deaths when chloroform was given for intended operations, or immediately before operations	18	22	14	54
Deaths during the progress of operations.....	22	6	14	42
Deaths after operations—*i.e.* from chloroform immediately after, or the result of chloroform and the operation combined a short time after the operation had been completed	6	12	7	25
In 133 deaths, the relative frequency of deaths as to Sex was:—				
Males.....	30	32	28	90
Females.....	20	16	7	43

at present about a hundred deaths from pure chloroform, and one-fourth (or twenty-five) from ether or amylene! Next, as to the stage or degree of the anæsthetic process which appears most dangerous, this (contradictory though it seems) is decidedly the stage of excitement, or the early stage of violent plunging, before the patient is rendered anæsthetic and fit for the operation he is about to undergo. This escaped the knowledge of Dr. Snow.

I think it will be found that the mode of death by "fits," trachelismus, syncope, &c., observed before operations, occurring suddenly, and consequently the means to be adopted for resuscitating patients in such accidents, differ very widely from the mode of death observed and means to be adopted for resuscitation of patients who show a tendency to sink after operations (by asphyxia?). We have hitherto confounded the two, and have been looking for some one great secret cause of these most melancholy occurrences. One set of accidents seem to occur suddenly from chloroform; the other slowly, but from ether. It appears also very instructive and consoling that in all the large tedious operations,—such as resections, large amputations, midwifery, ovariotomy, ligatures of large arteries, &c.,—such is the "law of tolerance" of chloroform, that in these formidable operations it is almost free from danger; whereas, in trivial operations (where, fortunately, we might use Dr. Arnott's congelation plan) the chances of accident are very serious indeed.—*Med. Times and Gazette, May* 12, 1860, *p.* 482.

126.—ON THE INFLUENCE OF BELLADONNA ON THE PNEUMOGASTRIC NERVE.

By Richard Hughes, Esq., M.R.C.S., and L.R.C.P.Ed. (Exam.), Surgeon to the Brighton Orthopædic Hospital.

Dryness of the throat and dysphagia are two of the earliest and most invariable symptoms of the influence of belladonna. On what do they depend?

The pneumogastric nerve appears to preside over alike the muscular fibre and the secreting apparatus of the alimentary canal from the fauces to the stomach. Claude Bernard has ascertained the result of its division upon the stomach. "The section was made during the free flow of gastric juice (though a canula previously introduced into the stomach), excited by the presence of an alimentary bolus. The flow immediately ceased, and the mucous membrane, which had been tense and turgid the moment before, became withered and pale. On introducing the finger into the stomach itself, the walls were perceived to be perfectly flaccid, and there was no longer the gentle pressure which had been previously felt." (Carpenter's Human Physiology, 5th ed., p. 87). It stands to reason that, if such be the effect of loss of pneumogastric influence on the stomach, similar phenomena—check of secretion and diminished muscular power—higher up in the ali-

mentary canal, must be referred to depression of the influence of the same nerve. Consequently the dryness of throat and dysphagia caused by belladonna must result from a depressant influence exercised by it on the pneumogastric nerve.

That belladonna has such an influence will farther appear from a consideration of its effects on disease. Hooping-cough and asthma are admittedly spasmodic affections, in which irritation of the pneumogastric nerve is at the bottom of the phenomena: and both of these affections are singularly under the control of belladonna and its congeners. Stramonium is the favourite remedy in asthma; and the use of belladonna in hooping-cough is becoming more and more general. In the former case, we have the evidence of direct experiment for our theory. Galvanisation of the pneumogastric has been found by Valentin to produce constriction of the trachea and bronchial tubes; while on the other hand, in animals poisoned by belladonna, and stramonium, these tubes have been found lax, and have refused to contract under the strongest stimuli (Watson's Lectures on Physic, 4th ed., vol. ii, p. 358, 363). Lately, moreover, an enterprising French surgeon has attempted and (apparently) achieved the cure of asthma by injecting a solution of atropine upon the pneumogastric nerve in the neck (Medical Times and Gazette, Nov. 26, 1859). Another affection in which belladonna has been found very beneficial, and in which the pneumogastric is the seat of irritation, is obstinate spasmodic vomiting. A patient was suffering from cancer of the pylorus; the usual incessant vomiting took place. Belladonna was given, and it ceased. After death the stomach was found lax and enormously distended. Mr. Amesbury tells me he once had a case of obstinate vomiting in pregnancy. All the usual remedies failed; at length he tried belladonna, with complete success. Here again, as the pneumogastric is the motor nerve of the muscular coat of the stomach, a sedative influence exercised by the belladonna upon this nerve will explain the phenomena.

The practical result from the above facts will be, that we shall be led *rationally* to the use of belladonna and its congeners in all affections of the pneumogastric nerve. Laryngismus stridulus must therefore be added to the above mentioned morbid conditions. I have lately used belladonna (in combination with nitric acid) in every case of common cough which has come under my notice, and with far more marked success than I have obtained from any other remedy. I trust that a more extended trial may add fresh support to the view here advocated.—*Brit. Med. Journal, May* 26, 1860, *p.* 393.

127.—*Opium and Belladonna Plasters.*—Mr. A. F. HASELDEN, having observed the inconveniences attending the use of these plasters of the Pharmacopœia, of which most medical practitioners are very sensible, (viz., that the preparation runs beyond the margin of

the leather, or frequently penetrates it, and stains unpleasantly the clothing,) proposes, in a recent number of the 'Pharmaceutical Journal,' methods of obviating them. He recommends the use of resin plaster instead of gum thus, and morphia in place of opium. For the opium plaster, he proposes the following formula, which has been successfully employed:—Lead plaster, six ounces; resin plaster, four ounces; acetate of morphia, one drachm; acetic acid and water, of each four drachms. For belladonna plaster, the following:—Lead plaster, six ounces; resin plaster, four ounces; atropine, two grains; acetic acid, two drachms; water, six drachms.—*Med. Times and Gaz. May* 12, 1860, *p.* 482.

128.—CASES ILLUSTRATIVE OF THE INFLUENCE OF BELLADONNA.

By Richard Hughes, Esq., M.R.C.S., L.R.C.P. Ed., (Exam.) Surgeon to the Brighton Orthopædic Hospital.

[Dr. Hughes in two previous papers, one in the 'British Medical Journal' for June 2, 1860, and the other in the 'London Medical Review' for August, 1860, enters into the physiological effects of this medicine. He thinks that it is an *excitant of the sympathetic* and a *depressant of the cerebro-spinal system.*]

In the following paper, says he, I propose to exhibit some results of the practical application of this theory, and to point out the farther indications for practice thereby afforded. And I will begin with cases where the action of this drug upon the sympathetic system appears to be the *rationale* of its curative influence.

Case 1.—*Enuresis.*—July 1859. George M., aged 14, had long suffered from nocturnal incontinence of urine. There was nothing particular about the general appearance of the patient, or that of his urine. I ordered small doses of tincture of cantharides, which relieved him temporarily; but immediate relapse followed its discontinuance. I now prescribed one-third of a grain of extract of belladonna twice a day, in cinnamon-water. In three days his complaint left him; a slight relapse, about a week subsequently, was immediately removed by a few more doses of the medicine; and he has since been entirely free of his ailment.

Remarks.—Incontinence of urine may obviously result from two immediate causes. 1. From over-action of the longitudinal fibres of the muscular coat of the bladder (detrusor urinæ). This is usually excited by some irritating quality of the urine, and is best removed by alkaline medicines and opiate suppositories. 2. From weakness or paralysis of its sphincter. This second and far more common cause of enuresis, usually met with in delicate children, requires for its cure some agent capable of toning the sphincter muscle, either immediately, or through the medium of its nerves. Such an agent

we have, according to my views, in belladonna; and, accordingly, the accounts of the influence of this drug over the disease from all parts of Great Britain and the Continent are highly encouraging. It rarely fails to overcome it, and in its use is subject to none of the evils occasionally produced by cantharides.

In prescribing this drug for children, the discovery of Dr. Fuller as to its comparatively slight effects on young subjects must be borne in mind; and what would seem to be large doses must be unhesitatingly used.

Case 2.—*Milk Abscess.*—July 1860. Emily H., aged 24, nine weeks after confinement caught cold. When I saw her, I found at the upper part of the left breast a hard swelling elevated above the surface, of about the size of a hen's egg; it was very painful, red, hot, and tender to the touch. There was also pain in the right side, constipation, headache, high fever; and, in the course of the preceding night, there had been some delirium. The fever had somewhat abated since vomiting a quantity of pure bile early in the morning. These symptoms pointed to concomitant derangement of the liver, and soon yielded to a mercurial pill and saline draught. The state of the breast I treated by suspending the mamma by a handkerchief tied over the shoulder, and keeping the swelling covered with the unguentum belladonnæ of the *Pharmacopœia*. In twelve hours, all pain had ceased, and there had been no increase of swelling. Next day, the redness and heat had gone, some tenderness only remaining. The swelling remained stationary and slightly tender for a few days, then slowly disappeared. The infant was not put to the breast during the mother's illness, and the general fever was sufficient to diminish the secretion of milk; but, as soon as she became convalescent, the flow of milk in both breasts returned as freely as before.

Remarks.—The localised inflammation of the breast to which (from its great tendency to suppuration) this term "milk abscess" is commonly applied, has now been frequently treated by belladonna. This drug has been ascertained to possess galactifuge properties; and accordingly, being applied in the form of extract or ointment around the nipple in these cases, it speedily checks the secretion of milk, and with it the inflammation.

The galactifuge properties of belladonna are easily explicable upon the theory of its action which I have advanced. Cl. Bernard has found that there are two ways of diminishing the secreting power of a gland—1, galvanising the sympathetic nerves which supply its blood-vessels; 2, dividing the cerebro-spinal nerves which supply its secreting cells. Belladonna, according to my view of it, both excites the sympathetic (as by galvanism), and depresses the cerebro-spinal influence (as by section) of the parts which it affects. The result of its application to a gland like the breast will therefore be to diminish alike its secreting power and the supply of the material on which that power is exercised. And this galactifuge property of the drug will be found

invaluable in cases of still-birth, premature weaning, and others, where the check of secretion of milk is loudly called for.

But it will be noticed that, in the case I have recored, there was no check of the secretion of milk. Not to speak of the superior practical advantages of a plan of treatment which avoids this unpleasant concomitant, it is obvious that the *rationale* of the cure must be a different one. It seems to me to resolve itself into the simple action of belladonna in exciting the vaso-motor nerves, and thereby contracting the capillaries. The essence of the inflammatory condition, to my thinking, is a morbidly dilated state of these vessels. The exudation from them, on which Professor Hughes Bennett and his school lay so much stress, is surely a consequence only of their preternatural dilatation and fulness. A substance which will contract them will save them the necessity of relieving themselves by exudation at all, and, even when this has taken place, will most surely promote its absorption. And this was the view of John Hunter. "If," he wrote in 1794, "we could discover an agent which would cause the contraction of the extreme vessels, we should have in this the true remedy for inflammation."

This case, then, of cure of threatened milk-abscess by the local application of belladonna, appears to open a wide field for the treatment of inflammation in general. The three indications which inflammation gives us for treatment are—1. To moderate the force and frequency of the heart's action; 2. To diminish the superfibrination of the blood; 3. To reduce the hyperæmia of the part affected. The old mode of effecting the first two objects—in other words, of relieving the symptomatic inflammatory fever—was to bleed, and to give mercury; which may fairly be described as robbing a patient of a portion of his life, and poisoning the remainder. A more enlightened practice fulfils the former indication by antimony, aconite, and the other substances which have this physiological effect upon the heart; and the latter by low diet, diluents, and salines. The third indication—the reduction of the hyperæmia of the part—is commonly met by local bleeding and cold. But these are measures only applicable in their fullness to external inflammations, and even there are open to much improvement. Local bleeding, however efficacious, wastes blood, and leaves the capillaries weak and still morbidly dilated; and cold must be continuously applied, to be of avail. We should gain immensely, could we find a remedy capable of acting, whether locally or through the general system, upon the nerves of the inflamed part, so that its dilated capillaries might be aided to that contraction for which Nature is striving, and thus the hyperæmia of the part reduced without the loss of a drop of blood, or the inconveniences inseparable from leeches, cupping, and the continuous application of cold.

Such an agent I believe we have in belladonna; and I augur an immense advance in our control over inflammation as its use therein becomes extended within the limits which practical experience alone

can define. (See the Lancet for Aug. 4, 1860, for a case of myelitis treated by belladonna applied along the spine.)

Case 3.—*Congestive Headache.*—August 1860. Mrs. M., aged 74, for four days in succession had been visited with severe headache, beginning with blindness, and lasting the greater part of the day. Similar headaches, some years ago, had been relieved by leeching. The administration of belladonna was commenced at the first coming on of the blindness, on the fifth day. The attack this day passed off sooner than usual. On the sixth day, there was but a slight attack; and since then there have been none.

Remarks.—The remarks made upon the last case will in most respects apply here, substituting congestion for inflammation. The congestive character of the headaches seemed marked—1, by the accompanying blindness; 2, by the relief formerly afforded by leeches; 3, by the absence of good effect from a full dose of Hoffman's anodyne—a medicine which I find invaluable in cases of simple nervous headache. Here the belladonna accomplished the same good end as the leeches, without their expense, inconvenience, or spoliation of the vital fluid.

The result of this case would seem to indicate the use of belladonna to remove that habitual tendency to congestion of the brain which usually results in apoplexy.—*British Medical Journal, Sept.* 8, 1860, *p.* 706.

129.—*Influence of Fatty Bodies on the Solubility of Arsenious Acid.* By M. BLONDLOT.—The remarkable fact, to which the author calls the attention of toxicologists, is the property which fatty bodies possess of presenting an obstacle to the solubility of arsenious acid, either in plain water, or in water slightly acid or alkaline. A large number of experiments have proved to him that the least contact of arsenious acid, in the concrete state, with a fatty body, is sufficient to reduce its solubility in these different menstrua to one-fifteenth or one-twentieth of what it would be, *cæteris paribus*, without the intervention of the fatty principle,—a fact which may easily be ascertained by testing the proportion of arsenic dissolved by means of starch and tincture of iodine. Since a mere trace of any fat whatever is sufficient to produce this effect, and since neither acids nor energetic bases oppose any obstacle to it, it is evident that there is not in this case any chemical combination between the arsenious acid and the fatty body; and that, therefore, the latter can only interfere mechanically by soaking the arsenious acid, so as to remove it from the action of the aqueous fluids, which would dissolve it. This fact, in itself so simple, is susceptible of numerous applications in toxicology. It explains, in the first place, how it has happened that, in medico-legal analyses, arsenic has sometimes been sought for in vain in the liquid portion of aliments which contained it, when they were more or less fatty, such as soup, milk, &c.

It also gives the reason why arsenic, taken in the form of powder, becoming mixed in the stomach with fatty substances, which retard its solution, has occasionally remained a very long time without producing poisonous effects,—a circumstance which might, in certain cases, mislead the investigations of justice. In the same way we may account for a very significant fact related by Morgagni, that, in his time, it was not very uncommon to see jugglers swallow, with impunity, pinches of arsenic, because, he adds, they had taken the precaution of swallowing beforehand milk and fatty substances, which they brought up again by vomiting after the public had retired. Finally, these experiments show the benefits which may be derived, in this kind of poisoning, from the administration of fatty bodies, especially of milk, which not only have the advantage of acting as emollients. according to the general belief, but which are veritable antidotes, capable of of retarding considerably the solution, and consequently the absorption, of the arsenious acid, which as often happens, still remained in the concrete condition.—*Acad. des Sciences.—Edinburgh Medical Journal, Aug.* 1860, *p.* 176.

130.—*On Peroxide of Hydrogen.* By Dr. RICHARDSON.—Dr. Richardson has been employed in this research for twelve months, and his observations in relation to the nature of the body in question, its formation for therapeutical uses, and its effects on animals, are singularly interesting. For instance, he showed by experiment that the oxidizing power of the solution of peroxide is suspended by the presence of all narcotics; thus establishing the great law advanced by Snow, that narcotism is suspended oxidation, and that every substance, which on being introduced into animals produces narcotization, has the property, either by a negative influence, or by catalysis, of preventing the union of oxygen with other substances with which it is in contact, and for which it has an affinity. Again, it was shown that some animal structures, to say nothing of certain inorganic bodies, on being brought into contact with the peroxide in solution, liberate the oxygen. Fibrine has this property, and carbonic acid. A fish placed in the solution evolved the oxygen with considerable action. Physiologically, the peroxide, on addition to venous blood, gives to the blood the arterial character; it stimulates the left side of the heart to contraction, but seems to stop the action of the right side. Injected into the arteries, it restores, for a time, a condition of muscle during which contraction occurs on the application of an excitant. It also suspends cadaveric rigidity, and further, it prevents the spasms of muscle caused by such bodies as ammonia and hydrocyanic acid. Therapeutically, peroxide of hydrogen offers itself in all cases marked by deficient oxidation; in low fevers; as an antidote to various poisons; in tetanus; in diabetes, and in cancer. It is given compatibly with all the mineral acids; and the doses of a solution charged

with ten volumes of the peroxide is from one increasing to four fluid drachms, in distilled water; externally, it forms a deodorising lotion. Dr. Richardson took pains in his paper to explain the exact mode of making the solution; but we feel that the process as yet is too complicated for general introduction into the dispensary. To bring the substance into general use, some practical Pharmaceutical Chemist must take it in hand. We are informed, indeed, that Bullock and Reynolds are already preparing the peroxide solution, and as several practitioners are only waiting for the proposed remedy to test its effects, we may expect soon to see its virtues fully brought out in the treatment of disease. It is just to say that the introducer of the remedy specially guarded himself from offering any extreme views: he claimed simply that a substance possessing such singular properties, physiologically, should be used rationally as a medicine in extreme cases for which we now have, virtually, no means at command. —*Med. Times and Gazette, Oct.* 20, 1860, *p.* 382.

131.—ON THE SIGNIFICANCE OF THE CONTRACTION AND AND DILATATION OF THE PUPIL PRODUCED BY OPIUM AND BELLADONNA RESPECTIVELY.

By Richard Hughes, Esq., M.R.C.S., L.R.C.P., Ed. (Exam.), Surgeon to the Brighton Orthopædic Hospital.

It is well known that the muscular fibres of the iris, by whose contractions the diameter of the pupil is enlarged or diminished, are arranged in two sets: a circular set, immediately around the margin of the aperture; and a more external longitudinal set, radiating from the centre to the circumference of the iris. It is obvious that while the contraction of the circular fibres will diminish the size of the pupil, that of the longitudinal fibres will enlarge it.

It is also now ascertained that these two sets of fibres are animated by two distinct nerves, emanating from different divisions of the nervous system. The circular fibres receive a branch of the third cranial nerve, and are therefore under the influence of the cerebro-spinal system; while the longitudinal fibres are supplied by filaments from the cervical ganglia of the sympathetic.

In consequence of this duplex muscular and nervous arrangement, the diameter of the pupil is always under ordinary circumstances, the same. So long as the normal influence of both third and sympathetic nerves is kept up, an equilibrium is maintained. On the other hand, let either of these channels of nervous supply, or their sources, be injured or depressed, and the balance is lost. The fibres animated by the unarmed nerve will obtain a predominance over the others, and passive contraction or dilatation will ensue, analogous to that which obtains in the lower extremities when, the flexors or extensors of the limb being paralysed, deformity is produced by the unbalanced influ-

ence of the opposing muscles. A similar loss of balance will follow the excitation of either of the animating nerves. The active supply of nerve force set on foot by the stimulus will overcome the influence of the mere passive conveyance kept up by the unexcited nerve, and active contraction of the fibres supplied by the other will be the result. Examples of the former mode of destruction of equilibrium are—

Contraction of the pupil from section of the sympathetic in the neck.

Dilatation of the pupil from diminished power of the third nerve in compression of the brain.

Of the latter mode—

Contraction of the pupil from reflex excitement of the third nerve produced by the stimulus of strong light acting through the optic nerve.

Dilatation of the pupil from galvanic stimulation of the sympathetic in the neck.

It follows from the above facts and considerations that the state of the pupil will at any time (except in cases of local injury) be an index to the condition of one or both of the two great divisions of the nervous system. The only question to be asked in each case is—have we here abnormal depression of one system, or abnormal excitation of the other? And the accompanying symptoms will generally enable us to answer this inquiry.

Reserving for a future paper the consideration of the states of the pupil in disease, I shall here inquire into the rationale of its appearances under the influence of certain drugs.

The state of the pupil is a marked feature of the influence of opium on the one hand and of belladonna and its congeners (hyoscyamus and stramonium) on the other. The latter produce dilatation of the pupil, whence they are called mydriatics: the former causes as decided contraction.

The question arises—Is this a casual difference, or is it an index to an essential diversity between the two classes of substances? By farther considering their respective actions, we shall see the latter to be the true statement of the facts.

1. Opium is a narcotic, *i.e.* produces cerebral unconsciousness going on, when large doses are taken, to coma. The mydriatics have no such effect; on the contrary, one of their poisonous results is delirium.

2. Both are anodyne; but this influence of belladonna is only seen in external neuralgiæ, while that of opium extends over the whole class of internal pains. For instance, we should not give belladonna in peritonitis, nor opium in tic-douloureux.

3. Opium checks all the secretions, save that of the skin, and constipates. The mydriatics in medicinal doses leave the secretions untouched, and hyoscyamus and belladonna (if not stramonium also) are gently aperient.

4. Young children, as is well known, are exceedingly intolerant of

opium; while, as Dr. Fuller has shown (Med. Times and Gazette, July 23, 1859), the younger the patient, the larger in proportion is the dose of belladonna required to produce its physiological effects.

Actuated by such considerations as these, it has been lately recommended to give opium as an antidote in poisoning by belladonna, and *vice versa*. I have seen no account of the use of belladonna in opium poisoning; but of the former practice we have at least one most successful set of cases. In the 'Med. Times and Gazette' of December 3, 1859, Mr. Seaton, of Leeds, records the history of ten persons who partook of the berries of the atropa belladonna. In two of these cases the symptoms were slight, and an emetic was the only thing required. In the eight others, the full poisonous effects of the drug were obtained, especially active delirium and wakefulness. Opium was given to all; and in seven of them the symptoms disappeared as soon as sleep and contraction of the pupil indicated its influence over the system. The eighth patient, who was in bad health at the time and took but little opium, died.

I believe that these facts ought to be more widely known, and put into practice, that facts may test what theory thus indicates, that opium and belladonna are directly antagonistic in their effects, and are, therefore, antidotes one to another.

Careful investigations and experiments are required to ascertain how far belladonna may be given to relieve symptoms which arise from opium poisoning, even in cases where they are otherwise caused, and *vice versa*. We know that the cerebral excitement of delirium tremens, which is precisely analogous to that produced by belladonna (and in which the pupil is ordinarily dilated), is completely under the control of opium. May not congestive headache, and coma with contracted pupil—however arising—be indications for the use of belladonna?

But we must be cautious here, and not proceed to give these drugs in fevers and other cerebral affections, purely on the evidence of the state of the pupil. As Dr. Pereira has remarked, all practical experience is against giving opium in certain affections (*e.g.*, compression of the brain) where nevertheless the pupil is dilated.

An interesting physiological enquiry seems suggested by the question, as to what is the process by which opium contracts, and belladonna dilates, the pupil?

We have seen that contraction of the pupil may be produced either by excitation of the third nerve or its source, or by depression of the sympathetic nerve or its source. It is *a priori* very unlikely that opium, whose full effect is depressant to the whole cerebro-spinal system, should act as an excitant to one nerve proceeding from that system. It is most probable, therefore, that opium contracts the pupil by such a depressant influence on the sympathetic nerve as that the third, still excited by the stimulus of light through the optic nerve, or even without this, is more than a balance for it.

This *a priori* probability must now be brought to the test of the observed facts of the action of opium. Will depression of the sympathetic influence explain these? I believe it will in nearly every instance.

We may enumerate its effects as follows:—1. Contraction of the pupil. 2. Congestion of the brain and other internal organs; but pallor of the skin. 3. Diminution of all secretions save that of the skin. 4. Constipation. 5. Slight cerebral excitement, followed by heavy sleep, merging into coma. 6. Relaxation of involuntary muscles. 7. Relief from pain.

Let us consider the above phenomena one by one, with a view to enquiry how far they can be accounted for by depression of the influence of the sympathetic nerve.

1. It has already been shown how improbable it would be to suppose any excitant action of opium upon the third nerve, and how therefore its contracting influence upon the pupil can only result from a depressing influence exerted by it upon the sympathetic.

2. The sympathetic being the motor nerve of the muscular fibre of the blood-vessels, it is obvious that depression of its influence will allow of an increased afflux of blood through the vessels. Such is the invariable result of division of this nerve as practised by C. Bernard, Brown-Séquard, and others. Now increased afflux of blood, if kept up for any time, becomes congestion. The congestive effects of opium, therefore, are sufficiently explained by its depressing influence on the sympathetic nerve. The pallor of the surface, however, seems an exception to this result: yet is not really so. For it is obvious that the whole body cannot be congested, while the amount of blood in it continues the same. What is gained by one part must be lost by another. In this case (as in the cold stage of ague) while the internal parts are congested, the external are drained.

3. While increased active afflux of blood, (as in the case of division of the sympathetic in the neck) sets up at first increased secretion, the congestion into which it soon merges has a precisely opposite effect. The urinary secretion is diminished in congestion of the kidney; but little bile is formed by a congested liver. So that the congestion of the internal organs produced by the depressing influence of opium on the sympathetic, amply accounts for the diminution in the secreting functions of these organs.

The truth of this explanation seems demonstrated by the fact, that the only organ which is not congested by opium—the skin—is also the only one whose secretion is not checked. Indeed, it is considerably increased; and this probably from the call which the congested kidneys make upon its vicarious function of aqueous elimination.

4. The diminished secretions of the intestinal canal would, in their turn, account sufficiently for the well-known constipating effects of opium. But I am inclined to think (from the opposite effects of bel-

ladonna) that this result is largely contributed to by the diminution of nervous excitement sustained by the muscular fibre of the intestines in consequence of the depression of the sympathetic's influence.

5. The effect of opium upon the cerebral hemispheres will, if the view I have propounded of the generation of nerve-force, be accepted, viz.: that it is secreted by the cells of the gray matter from the blood—be only a particular instance of its general influence upon secretion. The slight temporary excitement, and the subsequent gradual loss of consciousness—going on, when large doses have been given, to stupor and coma—are precisely analogous to the temporary increase of secretion from the active afflux of blood, and its subsequent diminution as congestion is established in other glands, whose blood-vessels have lost their sympathetic influence. The relaxation of the cerebral vessels produced by the depression of the sympathetic will, therefore, account for these phenomena also.

This is a highly novel conclusion, but is, I think, well borne out by the facts of the case. Natural sleep is, according to the generally received opinion, the result of a slight congestion of the brain. Habitual congestion—as in apoplectic subjects—induces habitual sleepiness. While the great internal congestion produced by severe cold, causes—as is well known to travellers—profound sleep which is often fatal. On the other hand, the theory that the soporific effects of opium depend entirely upon a direct sedative influence exercised by it upon the gray matter of the cerebral hemispheres, will hardly account for all the facts of the case. I allude especially to the primary mental excitement which follows its use, which could not exist, were its action upon the cerebrum directly sedative. And the excitement is too slight and fleeting to allow us to suppose the subsequent sleep the result of exhaustion or reaction, and so to class opium with alcohol and the inebriants.

6. The relaxation of spasm of involuntary muscles is a well-known effect of opium; as in the passage of gall-stones through the ductus choledochus, retention of urine from spasmodic action of the urethra or sphincter vesicæ, colic, &c. Now, all these involuntary muscles are animated by the sympathetic nerve; and their relaxation by opium falls thus under the general head of its depression of sympathetic influence. On the other hand, in asthma, which is a spasmodic affection of muscular fibres supplied, not by the sympathetic, but by the pneumogastric nerve, opium is (according to our latest and best authority on this disease, Dr. Hyde Salter), of little or no use.

The accounts given by authors of the state of the voluntary muscular system under the influence of opium are so various and contradictory, that I have no inclination to attempt a rationale of facts themselves so uncertain.

7. There is one more effect of opium—its anodyne influence; and this alone, I think, cannot be accounted for by its depressant action on the sympathetic. We must here allow a similar depressing action

upon the centres of sensation; the gray matter of the cord and the sensory ganglia at the base of the brain. And it is noticeable that in diseases accompanied by severe pain, the whole energy of the opium seems expended upon these sensational centres. The relative demand conditions the direction of the supply, and its physiological results from depression of the sympathetic—even contraction of the pupil—are not seen. Dr. Todd remarks this in respect of acute rheumatism; of one case he writes, "Thus, in 48 hours she took 16 grains of opium, exclusive of a grain and a half of morphia on each night, yet her pupils were not at all contracted, nor was she in the least degree narcotized. The effect upon her has been most beneficial: her nervous excitement has been calmed down, and her pain materially relieved." ('Clinical Lectures on Acute Diseases,' p. 47). So Dr. Sibson writes ('British Medical Journal,' Aug. 13, 1859,') of the effect of frequent and full doses of opium in this painful disease. "The first thing that suggests itself for notice is the remarkable absence of any symptoms, such as accompany the administration of opium in less painful disorders. We hear hardly anything of drowsiness, of tendency to coma, of constipation, or contracted pupils; only, when the remedy is pushed to its extreme point, delirium comes on, which speedily subsides under the diminution of the dose. In some cases, so far from constipation being produced by the opium, the patient has actually suffered from diarrhœa during the opium treatment. In most cases, there has been improvement in the patient's sleep; but this has never proceeded to the extent of narcotism. The pain was in almost all cases much relieved."

It may appear strange to admit a sedative influence of opium upon the sensory portion of the gray nervous matter, and to deny it such an influence on the same tissue in the cerebral hemispheres. But a similar thing obtains with woorara, which affects the motor portion of gray matter only, leaving its sensational and ideational regions untouched.

The last evidence I have to bring forward as to the specific influence of opium on the sympathetic nerve is drawn from its use in the treatment of ague. I purpose shortly to publish facts and reasonings which tend to show that the "cold stage" of this disorder is the result of irritation of the sympathetic nerve by the malarious poison, and the "hot stage" the morbid re-action of the contracted capillaries. Now, opium, if given immediately before the cold stage, will often cut short the whole paroxysm; which would obviously be the case if its action be sedative upon the very nerve whose irritation causes the phenomena.

Farther, the same considerations have led me to reduce the curative influence of quinine in intermittent fever under the general head of its tonic action upon the vaso-motor (*i. e.* sympathetic) nerves. Quinine, therefore, is a tonic where opium is a sedative; and we should expect to find these two drugs antagonistic; and that such is the

case would appear from a remarkable experiment by M. Gubler. He states that "while opium causes cerebral congestion, quinine possesses a diametrically opposite influence. In support of this theory, the ingenious author relates a case in which 30 grains of quinine and 5 grains of extract of opium simultaneously administered, failed to produce either the characteristic intoxication of quinine, or the somnolency to be expected from opium."—'Medical Times and Gazette,' July 3, 1858.

It has thus, I think, been established, that opium contracts the pupil, and produces well-nigh the whole of its physiological effects, by a depressant influence exerted by it upon the sympathetic nervous system. The next question is, do belladonna and its congeners dilate the pupil by an opposite action upon the same sympathetic nerve?

The first evidence that such is the case arises from the fact, that belladonna causes contraction of the small arteries. Dr. Brown-Séquard, in his work on Epilepsy (see also his Lecture on Paraplegia, Lancet, May 5, 1860,) states this to be an effect of its internal administration; and Mr. Wharton Jones (Prize Essay on Inflammation in 'Guy's Hospital Reports' for 1850, pp. 8, 9,) finds that the application of a solution of sulphate of atropia to the small arteries causes constriction of their calibre, while opium largely and at once dilates them. Now, as the sympathetic is the motor nerve of the blood-vessels, it is most probable that substances which cause their constriction, do so by a stimulant influence exerted upon this nerve. Constriction of the capillaries is a well-known result of galvanic excitation of the sympathetic in the neck.

But let us consider the several results of the administration of belladonna, with a view to ascertain how far they may be accounted for on the hypothesis of a stimulant influence upon the sympathetic system. They may be enumerated as—1. Dilatation of the pupil, with dimness of vision and protrusion of the eyeball. 2. Dryness of the throat, with dysphagia. 3. No congestion of the brain or other internal parts; the skin often hot and red. 4. The secretions somewhat diminished; the skin usually dry. 5. Slight aperient action. 6. Wakefulness and restless excitement, going on in severe cases to delirium. 7. Toning of most involuntary muscles. 8. Relief from pain.

1. Of dilatation of the pupil I need say nothing: and the dimness of vision may result from this, or from the diminution of vascular supply to the retina from constriction of its blood-vessels. The protrusion of the eyeball (noticed in most of Mr. Seaton's cases) is interesting; as it is one of the effects of galvanization of the sympathetic in the neck, enumerated by Dr. Brown-Séquard.

2. The dryness of the throat and dysphagia caused by belladonna, I have discussed in a paper which appears in the 'British Medical Journal' of May 26, 1860. I have there shown it to depend upon a specific depressant action exerted by this drug upon the pneumo-

gastric nerve, which presides over the secreting function and muscular power of the alimentary canal from the fauces to the stomach. This, of course, is a practical rather than a physiological conclusion; for the pneumogastric nerve can hardly be influenced by a substance which has no power over other nerves of the same nature. It will suffice for our present purpose to conclude that belladonna depresses that cerebro-spinal influence which presides over the function of secretion.

Will this help us in any way to get at its action upon the sympathetic? I think it will. The researches of Claude Bernard upon the salivary glands appear to establish the following as the law of the relation of the two divisions of the nervous system to the secreting glands:—That a branch from the cerebro-spinal system is distributed to the secreting cells, and a branch from the sympathetic system to the coats of the arteries; and, consequently, excitation of the former increases, of the latter diminishes secretion. The extension of this law to other glands, though probable, has yet to be established by direct experiment; but there can be little doubt that it obtains with that secreting surface over which the pneumogastric presides. For, as in the experiment cited in the paper I have referred to, division of the pneumogastric nerves has been found to produce immediate draining of the vessels and check of secretion in the parts which it supplies, while we have formerly seen that division of the sympathetic produces precisely opposite effects, viz.,—increased afflux of blood and secretion in the parts supplied by it.

If, then, the influence of the pneumogastric and sympathetic nerves be antagonistic, it seems to follow that belladonna, which exercises a depressant influence on the former, will have an excitant action on the sympathetic.

3. The absence of congestion will naturally follow from the contraction of the arteries produced by excitation of the sympathetic. In Mr. Seaton's cases, nothing peculiar was noticed about the skin; but in a more severe case reported by Mr. Holthouse (Medical Times and Gazette, Dec. 17, 1859), it was "pungently hot and dry." The same remark is made relative to a case of belladonna poisoning, recorded in the Edinburgh Medical Journal, April, 1860; and to three cases of poisoning by stramonium, contained in the British Medical Journal of October 2, 1858. Here again the opposition to opium is noticeable, in poisoning by which the skin is cool and pale; and the explanation there given may apply to this phenomenon also.

4. The effect of galvanic excitation on the sympathetic is to check secretion from the great diminution of the flow of blood to the parts. And such appears to have been the effect of belladonna in Mr. Holthouse's case, and of stramonium in the three cases of poisoning by this substance above referred to. In the former instance no urine was passed from 9 a.m. to 10 p.m., or 13 hours. In Mr. Seaton's cases the secretions are not mentioned. Medicinal doses rarely have

any appreciable effect on the secretions; but in Dr. Fuller's cases of administration of belladonna the urine is said to have been scanty.

5. While opium is the most powerful agent we have for locking up the bowels—which we have seen that it accomplishes by diminishing the secretions and depressing the nervous powers of the intestinal canal—the mydriatics have on the other hand (in medicinal doses) an aperient action. This has not indeed been ascertained with regard to stramonium; but it is well known in the case of hyoscyamus; and I can quite confirm Dr. Fuller's statement of the aperient power of belladonna. I have known half-a-grain of the extract, taken twice a day, produce in four or five days quite troublesome purging in an adult whose bowels were usually costive. Such a result would naturally follow from gentle stimulation of the sympathetic which animates the muscular fibres of the intestines.

6. The most marked feature of large doses of the mydriatics is the wakefulness and restless excitement going on to delirium, to which they give rise. In Mr. Holthouse's case the symptoms were precisely those (to use his own words) of delirium tremens. Now the *post mortem* investigation of this disease invariably shows, according to Mr. Solly (On the Brain, p. 352) a bloodless condition of the gray matter of the cerebral hemispheres. And just as a great diminution in the supply of blood to the motor centres of the brain cause—as shown by the beautiful experiments of Kussmaul and Tenner—convulsions, so it would seem probable that a similar sudden anæmia of the intellectual centres may cause their analogue—delirium. And such a sudden anæmia would be caused by the contraction of the cerebral arteries resulting from excitation of the sympathetic. The delirium produced by the mydriatics may thus be satisfactorily accounted for.

A state of coma may ultimately come on in cases of poisoning by belladonna and its congeners. In this case, it is of that form called by Mr. Solly "anæmic coma;" and has been frequently observed at the close of exhausting diseases (see Solly on the Brain, p. 361; Watson's Lectures on Medicine, 4th ed., vol. 1, p. 456; Gooch on Diseases of Women and Children, New Sydenham Society, p. 179). And there, as here, the pupil is dilated; not, as in coma from opium poisoning, contracted.

7. The toning effects of belladonna upon involuntary muscles are best exemplified in the now well-known power it has of curing incontinence of urine. This it would do by increasing the tone of the sphincter vesicæ, just as opium relieves spasmodic retention of urine by relaxing that muscle. And it is not improbable that the beneficial effect of a belladonna plaster applied over the region of the heart in palpitation of that organ from debility, may result from its toning rather than sedative effects upon the muscular fibre. In both cases, excitation of the sympathetic nerve which animates these muscles will account for the phenomena.

I have said "*most* involuntary muscles," because some are supplied not from the sympathetic, but from the cerebro-spinal centres. I may instance the muscular walls of the respiratory tubes and the alimentary canal from the fauces to the stomach. And here belladonna is found to have a precisely opposite effect (as is shown in my paper on its influence upon the pneumogastric nerve), and relaxes spasm rather than increases tone.

8. But what shall we say of the anodyne influence of belladonna? can that be brought under the general head of sympathetic excitation, or must it be referred to that sedative power over the cerebro-spinal system which we have seen it—as in the case of the pneumogastric—to possess? There can be little doubt that the latter is the true explanation. But to distinguish it here from opium, we have the fact I have before noted, that belladonna is especially beneficial in external neuralgiæ, where particular nerves are affected: in which cases we rarely give opium with advantage. I am inclined to think that this anodyne influence of belladonna is local acting upon the affected nerve or nerves, rather than general on the nervous centres. Its great value when subcutaneously injected over the course of the nerve in sciatica, has been well ascertained (British Medical Journal, Feb, 11, 1860); while its internal administration in the same disease is productive of far less benefit. And as to its effects on the urinary system, I would quote one of Dr. Fuller's conclusions, derived from the result of the administration of belladonna in full doses to children. "That inasmuch as it exercises a remarkable power in affections of the urinary organs, and the experiments recorded in this paper prove that it is excreted with the urine, it is highly probable that its curative action in such cases may be due in a great measure to its topical effect, and if so, that it might be applied locally with advantage."

I have in the above paper pursued into some detail the thoughts which arise from the fact that opium contracts, and belladonna dilates the pupil. The theories advanced are in many cases novel, and require much discussion to establish them. I can only say, that at present they seem to be the best to account for the facts.

In a future paper I hope to investigate the rationale of the state of the pupil in various morbid conditions, and the indications derivable therefrom.—*London Medical Review, Aug.* 1860, *p.* 92.

132.—*On the Physiological Effects of Opium.* By GEORGE BODINGTON, L.R.C.P.E., Sutton Coldfield.—Of course, it is nothing new to remark, that in delirium tremens, very large doses of opiates may be given, not only with impunity, but with almost certain advantage. I will not say what quantity of morphia may have been given, as that is the more dangerous form of the drug, but that from two drachms up to six of laudanum per dose has been often attained in delirium tremens is well known to experienced practitioners. Under what

circumstances, then, does it occur that the drug so administered becomes comparatively innocuous? It is when the system is, so to speak, saturated with alcohol. On the other hand, the smallest dose given to patients who are mere water or tea and coffee drinkers, exclusively of alcoholic beverages, has an almost instantaneous and very powerful effect. Here is an instance in point. A lady, entirely a teetotaller, was seized with a catarrh and with severe coughing. An eight-ounce almond emulsion was prescribed, containing only about half a grain of morphia, of which she had taken about a fourth or a third part, when she fell into a complete state of syncope, and was restored by the dashing of cold water in her face and by the exhibition of stimulants. These occurrences are, I believe, frequent in general practice, and well known to the profession. The point appears to be this. Had the lady in question been in the daily habit of drinking a moderate quantity of wine or of other alcoholic beverage, no such sudden effect from so small a dose of morphia would have ensued. Contrasting this case with the effects of opium in cases of delirium tremens, the controlling powers of alcohol over the physiological effects of the drug are obvious.

It seems, then, that the use of the drug should be guided especially by the previous habits of the patient. The modern salts of the drug seem to be the most dangerous form in which it can be given. The native opium contains constituent ingredients of a contrary character which are counteractive of each other. Laudanum, holding all these in solution, is the safest form; inasmuch as the alcohol, the solvent, in some measure acts as an antidote. In practice, contraindications to either of these forms of exhibiting the drug frequently are present. In the treatment of lunatic patients, with whom the use of sedatives is of the first importance, my practice is to give Battley's sedative drop in preference to any other form of opiate, in a little brandy and water or sherry wine, this vehicle obviating the tendency to prostration or syncope which in some constitutions and under certain conditions might ensue. I am not aware that morphia administered in a similar manner would not have as good an effect.—*British Medical Journal, June* 9, 1860, *p.* 445.

133.—ON OPIUM AS A REMEDY IN POISONING BY DATURA.

By Dr. Thomas Anderson, Assistant Surgeon, Bengal Army.

[The antagonism of opium and belladonna has been insisted on by several writers within the last few years. Dr. Anderson considers that this doctrine was first propounded by himself.]

In Britain, poisoning by belladonna, or by henbane, is so exceedingly rare an accident, that it was vain for me ever to expect to meet with a case of either. However, in 1854, I proceeded to India, where

poisoning by datura is of everyday occurrence; and I determined to test my theory by administering opium to the first suitable case I should have an opportunity of treating. From the unsettled state of the country during my residence in India, first from the Santhal rebellion, and lately from the annexation of Oude, followed by the mutiny of our army, my service has been entirely military; and thus I have been thrown among the class of natives least obnoxious to datura poisoning. Only one favourable case has occurred to me, though I have heard of several cases that recovered, and assisted Dr. Fayrer, of Lucknow, in making *post-mortem* examinations of three or four individuals, to whom a fatal dose had been administered.

As this solitary case, however, illustrates my theory of the impossibility of these two classes of poisons exerting their action simultaneously on the brain, and is moreover, the converse of my former experiments, and at the same time supplementary to Mr. Bell's, I am induced to relate it in detail.

The 43rd Bengal Native Infantry, of which I was then the medical officer, while on its passage ftom the Punjab to Calcutta by boats, on the River Ganges, halted for four or five days at Futteyghur, in the beginning of December 1855. The sepoys were of course allowed to go on shore, and many of them visited the large bazaar in the native city of Furrackabad, two or three miles from Futteyghur, the British cantonment. Two days after our arrival, one of the sepoys, a man well known to me, was found by his comrades lying by the roadside, near Furrackabad, in a state of high delirium, and was brought to the hospital boat. On inquiry, I learned that he had been seen partaking freely of sweetmeats (the Hindoo eats them by pounds weight at a time) in the Furrackabad bazaar, some hours before he was found; and the men at once ascribed his condition to poison administered in the sweetmeats. The many varieties of native sweetmeats poisoned with datura and bhang, one of the preparations of Indian hemp, are well know in all large Indian towns, and are used to cause death or stupefaction, so complete, as to allow of robbery or violence being committed; and a sepoy, when in undress, usually displays valuable ornaments, sufficient to tempt a thief or thug. Even without this knowledge, the symptoms were so marked, that I had no difficulty in forming my opinion of the cause of the patient's condition. I saw him immediately after he was brought to hospital, and found him in the following state: He was reclining on his back in bed, in a wakeful muttering delirium, unbroken by an interval of even transitory consciousness. His face was much flushed; the eyes were reddened and wandering, and the pupils were brilliant, widely dilated, and quite insensible to light. The pulse was much accelerated and small. He continually twitched his extremities, but especially his hands, and every now and then pinched and tugged at the bedding. Though utterly unconscious to real external objects, the brain was evidently active, with continual visions before the eyes, as his mut-

terings and frequent clutchings referred to imaginary objects. He was not in the least degree violent, and required no restraint, but merely an attendant to keep him covered during his restless tossings about.

He swallowed any liquid put into his mouth, but exactly as patients in delirium tremens do, in one convulsive mouthful.

In an hour after his arrival at the hospital, I began the administration of opiates, and prescribed one grain of the muriate of morphia in solution, to be given every hour, beginning the first dose at 2 p.m. I watched the effect most closely, taking the state of the pupil as my principal guide.

Eight doses were given before I could observe any result. After the eighth dose, about 11 p.m., I noticed that his attention could be fixed for a moment, and that the muttering could be arrested by loudly talking to and shaking him; that the hands were less tremulous.

Still, no impression had been made on the widely-dilated pupils, and he was evidently as wakeful as ever. I therefore, before retiring for the night, directed the continuance of the morphia until the morning, with orders to discontinue it so soon as sleep threatened to come on. I did not see him again until six in the morning, when I found all his delirium gone, the tremulousness much relieved, the pupils almost reduced to their natural state, and the patient surrounded by his delighted comrades, who were listening with wonder to the native doctor relating to them how "dhutona" poisoning had been cured by "apheem" (opium.)

Though he was able to talk, and was nearly well, I considered it advisable to persevere with the treatment until sleep had been obtained. This did not supervene until three doses more had been given.

After several hours' sleep, he awoke perfectly well; and after two days more detention in hospital, he was dismissed "fit for duty;" and while I remained with the regiment, was never again in hospital. In all, fifteen grains of the muriate of morphia had been administered in eighteen hours; and with reference to the largeness of the amount, I took care to inquire concerning the habits of this sepoy, and found that he used opium in no form whatever.

In this case, as well as in the experiments published in my former paper on this subject, the amount of the narcotic used as the antidote, whether belladonna or opium, to restore the normal condition of the brain, would, by itself, have been a poisonous dose. This tolerance of the one poison, produced by the presence of the other poison in the system, seems to me to be a strong additional argument in favour of my theory.

It also induces me to go further, and to believe, that all narcotic poisons with distinctly opposite actions, and destroying life by their effects on the nervous centres, will, when present in the body

together, counteract each other, until eliminated from the system by the excretions.

For example, the distinct coma-producing narcotics, such as opium, will, I believe, in all cases be counteracted by the poisonous solanaceæ; and, as a remedy to the action of strychnia, I would hope for success from the use of conia, or preparations of hemlock itself; and, of course, in all these cases the opposite would hold true.

Unfortunately, no correct conclusion can be drawn from experiments made on the lower animals, as they are variously affected by narcotics. To dogs and cats I have given morphia in enormous doses without producing almost any effect; and to them five and six grains of atropia prove quite harmless. Rabbits will feed for months on belladonna leaves, and have their pupils dilated all the time, but otherwise they are not affected. Blackbirds eat belladonna berries in large quantities, and do not die.

It is, therefore, only to experiments on man that any value can be attached.

In conclusion, I would request my medical brethren never to give up hope of the most desperate case of opium-poisoning; but after the failure of the usual means, such as emetics, the stomach-pump, electricity, &c., to endeavour to produce, as rapidly as possible, the first symptoms of the action of belladonna on the system, by the administration of atropia, either internally or by injection under the skin, as indicated by Mr. Bell. My firm conviction is, that, in all such desperate cases (desperate as regards the old treatment), this plan, when boldly adopted, will be invariably successful.

Since writing the above, I have just heard that Dr. Carnegie, medical missionary in China, in a letter lately received from him, mentions a case of poisoning by opium which he had just cured by giving belladonna.—*Edinburgh Medical Journal, June* 1860, *p.* 1100.

134.—TANNIN AS ANTIDOTE TO STRYCHNINE.

By Professor Kurzak, of Vienna.

From want of a reliable antidote, the treatment in cases of poisoning by strychnine hitherto consisted principally in endeavouring to evacuate the poison, to combat the frightful spasmodic symptoms by narcotics, and to re-establish respiration, when it finally ceased, by artificial means. Donné proposed iodine, chlorine, and bromine as antidotes to strychnine; Garrod, Rand, Morson, and Falck recommended prepared animal charcoal; but the efficacy of these substances has been neither tested sufficiently by experiment nor proved by experience. The same is true in regard to tannin, and the astringent vegetables containing it, their infusions, decoctions, &c. Although they recommended themselves by the fact that tannin forms chemical compounds, insoluble in water, with strychnine and other poisonous

alkaloids, it seemed very probable that these products might be redissolved in the stomach and intestines, and thus be rendered capable of absorption; the virtue of tannin as antidote to strychnine was, therefore, considered very doubtful.

With a view to subject this matter to a thorough examination, and to ascertain the efficacy of tannin in preventing and allaying the symptoms of poisoning by strychnine, Professor Kurzak made a series of experiments on rabbits and dogs. At the end of his interesting and highly important memoir, he states that the results of his investigation permit him to draw the following conclusions:—

1. *Tannin*, if administered in time, *is an* excellent *chemical antidote to strychnine.*

2. The doubt, whether the precipitate formed by tannin in a solution of strychnine, although insoluble in water, would not be redissolved by the gastric and intestinal juice, and the strychnine thus reobtain its poisonous properties, is solved by these experiments on rabbits and dogs in a complete and highly gratifying manner.

3. The successful results in dogs and rabbits justify the expectation that *tannin would suspend the poisonous action of strychnine also in man*, even in cases where the evacuation of the tannate of strychnine, formed in the stomach, could not be accomplished.

4. These experiments show that *twenty to twenty-five times the quantity of tannin is required in order to suspend the poisonous action of strychnine.* In cases of poisoning it will be, however, advisable to administer a relatively larger proportion, as a part of the antidote will be absorbed by the usual contents of the stomach, particularly by gelatin.

5. As tannin has proved to be an antidote to nitrate of stychnia, which is much more soluble in water, there is so much greater reason to hope that it will be successful in poisoning by pure strychnia which dissolves in water with great difficulty.

6. *The same successful result* is to be expected from its administration in poisoning by the hard and tough *nux vomica*, which imparts the poison to aqueous fluids, but gradually and not very rapidly.

7. Tannin is a so much more valuable antidote in poisoning by strychnine, as *galls* in which it is contained can be readily procured, and thus be administered without much loss of time. They are easily reduced to a powder, which is given mixed with water. Another advantage is obtained by the vomiting which it is liable to produce. In the mean time, an *infusion* or *decoction* of powdered galls may be prepared.

On an average, Turkish galls contain fifty, the Illyrian galls twenty per cent. of tannin. At least one drachm of the former and two drachms and a half of the latter are therefore required to neutralize one grain of strychnine introduced into the stomach, but in general, especially if there is vomiting, a much larger quantity should be administered.

8. Another readily obtained substance containing tannin is *Chinese*

tea, the efficacy of which, in poisoning by strychnine, is confirmed by by our experiments. But these experiments (VII. and VIII.) have also shown that, in a decoction of tea-leaves, we cannot count upon the whole amount of tannin contained in them. In poisoning by a larger dose, it would therefore be necessary to administer so large an amount of green tea that the antidote itself might produce poisonous effects. One decigramme (1·3 grain) of nitrate of strychnine requires, as our experiments prove, ten drachms (600 grains, 40 teaspoonfuls) of green tea, which, according to Peligot's analysis, contain about fifteen grains of caffein. Tea is therefore applicable only in poisoning by smaller doses, but may otherwise be useful as adjuvant.

9. The efficacy of *roasted coffee* as chemical antidote to strychnine seemed to be much inferior. The amount of *caffeo-tannic acid* contained in coffee is, according to Payen, 3·5 to 5·0 per cent. But our experiments (IX., X., XI.) show that the decoction evidently contains a much smaller quantity of undecomposed tannic acid than this percentage would justify us in assuming. The decoction of 180 grains of roasted Cuba coffee (being adequate to 200 grains of the raw coffee, which should contain at least six grains of tannic acid) produced, according to the ninth experiment, merely a delay and dimunition of the poisonous effect of 0·13 grain of nitrate of strychnine. In the tenth and eleventh experiments, 300 grains of raw coffee, which weighed, after roasting, 267 and 264 grains, and should have contained at least nine grains of tannic acid, had furnished a decoction which, as antidote to 0·13 grain of strychnine, was nearly inert, only delaying the appearance of the symptoms for a little while.

10. From *unroasted coffee*, so inconsiderable an amount of tannin is extracted, by boiling, that the employment of its decoction for our purpose is out of question.

11. *Oak bark* (of Quercus robur and Q. pedunculata) contains, according to Gerber, 8·5 per cent. of tannic acid, and imparts it readily to aqueous fluids. It deserves attention in poisoning by strychnine so much the more, as it can be procured without much delay, especially in the country. What has been said about the administration of galls equally applies to the use of the powder and decoction of this bark.

12. On account of their frequent occurrence and the large amount of tannin they contain, we have to mention in this connection: *acorns* (from Quercus robur and Q. pedunculata) with 9 per cent., *the bark of the horse-chesnut* with 8 per cent., *willow bark* with 5½ per cent., and *the green hull of walnuts*. The *radix tormentillæ* (with 17 per cent.,) *rad. caryophyllatæ* (with 31 per cent.,) and *rad. bistortæ*, are still richer in tannin, but can rarely be procured without much loss of time.

13. The solubility of the precipitate, produced by tannin in a solution of strychnine, by *acetic*, *citric*, and *tartaric acid*, (vide experiments with the same,) show the necessity of avoiding vegetable

acids during the treatment of poisoning by strychnine with tannic acid.

14. The same applies to the internal use of *alcohol* and *alcoholic remedies.*

15. The reported experiments on rabbits have sufficiently proved that more active voluntary movements excite the spasms usually produced by strychnia, even when they otherwise would not have made their appearance. In treating cases of poisoning by strychnine, *it is therefore highly important to prohibit as much as possible all voluntary movements, and to avoid violent excitement of any other kind.*—(*Zeitschrift der K. K. Gesellschaft der Aerzte zu Wien*, March 12, 1860.)—*North American Medico-Chirurgical Review, July*, 1860, *p.* 744.

135.—*Detection of Phosphorus in Cases of Poisoning.*—SCHERER has found that the smallest trace of phosphorus may be detected by its reaction with a salt of silver. Phosphorus volatilises even under liquids, at the ordinary temperature, and a piece of paper moistened with nitrate of silver is blackened when suspended over it in a closed vessel.

In order to guard against error, it is necessary, when the fluid contains organic matter, to mix it with pure sulphuric acid, and suspend over it a piece of paper moistened with an alkaline solution of nitroprussiate of sodium, which would detect the presence of phosphoretted hydrogen by the violet colour produced. If by long contact with air the phosphorus has passed into the state of phosphorous acid, it may still be detected by distilling the liquid with zinc and sulphuric acid, and passing the evolved gas into a solution of nitrate of silver. The phosphoretted hydrogen present produces a black precipitate, which may also be oxidized with nitric acid, and then examined for phosphoric acid in the usual way.—*London Medical Review, July*, 1860, *p.* 46.

136.—*Examination of the Superficial Portions of the Eye by means of Natural Light Laterally Reflected from a Double Convex Lens.*—M. Laborie lately read a paper before a medical society of Paris, wherein he speaks of the advantages of the opthalmoscope, and those of the lamplight reflected laterally. He, however, finds fault with the latter mode of illumination, and prefers the natural light, reflected from a double convex lens, held in the hand of the surgeon, an inch and a half from the patient's eye. In this manner he has been enabled to examine affections of the cornea, of the anterior chamber, the iris, the pupil, and the lens. The great comfort is, according to the author, that the examination may be made at once, without repairing to a darkened room, or having recourse to artificial light, &c.—*Union Médicale; Lancet, May* 26, 1860, *p.* 525.

137.—*Recovery from Strychnine Poisoning.*—A very good paper on a case of recovery from poisoning by strychnine, was lately read by Dr. PART, of Camden Town, to the North London Medical Society. This respected physician succeeded in his case, by the use chiefly of emetics and cold affusion. "As the vomiting continued, so the spasms appeared to abate in their intensity," and in about eleven hours the effects of the poison had passed off.

Dr. Part, after reviewing the various antidotes which have been recommended, such as tobacco, woorara poison, chloroform, hydrocyanic acid, camphor, animal charcoal, tannin, tincture of iodine, and fat, recommends that we should first employ emetics, and then judiciously assist by chloroform. As soon as the vomiting has ceased, he strongly recommends camphor suspended in almond emulsion. In his case he considered that the cold affusion materially assisted in inducing inspiration. As emetics, he prefers sulphate of zinc with copious draughts of mustard and water.

138.—TWO CASES OF NARCOTIC POISONING; SUGGESTING A MODIFICATION IN THE USE OF THE STOMACH PUMP.

By J. EVELYN CROOK, M.D., Northfleet.

[The following cases prove that it is not necessary to *pump* fluid from the stomach, any more than from the urinary bladder, as both viscera may be emptied merely by passing a tube.]

Case 1.—May 15th, 1857.—An infant only twenty-two days old, had about forty minims of laudanum poured down its throat. The occurrence happened in this way:—The mother, after her confinement, was attacked with muco-enteritis; and, among other remedies, an opiate injection was sent to the house, part of which only was administered, as the patient quickly recovered. It appears that an elder child, seven years of age, was left alone with the infant, and she deliberately took the bottle containing the remainder of the opiate injection from the mantel-piece, and poured its contents down the infant's throat. The accident was soon discovered, and I was sent for; but not being at home at the time, did not arrive till the poison had been taken about half an hour.

When I saw the child, two mustard emetics and some antimonial wine had been given (how much I could not ascertain), but no vomiting had taken place. The pupils were contracted; the skin was cold; indeed, the child appeared to be dying. I immediately sent for my elastic male catheters, and passed a large one (No. 12) down the œsophagus. I had no sooner done so, than the contents of the stomach flowed through the instrument, and were collected in a basin.

I filled my mouth with warm water, and by applying my lips to the end of the catheter, forced the water into the stomach; the fluid was immediately returned through the tube. This was repeated several times. I afterwards injected some warm coffee, and withdrew the instrument, the child being all this time in the upright position. The little patient was then enveloped in flannels wrung out in water tolerably warm; and at frequent intervals cold water was dashed over its body. This treatment was continued many hours, the infant during the time being fed with milk and brandy. Twenty-four hours elapsed before it was again able to take the breast. It recovered, and is now alive and well.

Case 2.—On the 23rd of last November, a discharged soldier from India had been lying at the railway station in a state of coma for seven hours. At first, he was supposed to be intoxicated, so that little notice was taken of him, until a medical man, a passenger by one of the evening trains, pronounced him to be dying, and advised the police to be sent for. He was then removed to a public-house, where I attended him. I found him perfectly insensible; the pupils were contracted; the pulse was scarcely to be felt; the extremities, and indeed his whole body, were very cold. Friction, hot bricks, hot bottles, hot blankets, &c., were applied: but as the police strongly suspected him to be drugged, I considered it my duty (notwithstanding the time that had elapsed) to empty the stomach. I therefore passed the œsophageal tube into the stomach, having the syringe attached, and injected nearly a pint of warm water, when the man vomited, the contents of the stomach passing *by the side* of the tube. I then injected about another pint, and removed the syringe, the fluid then flowed through the tube into a basin. This was repeated until the water was returned perfectly clear, when strong coffee was injected, and the tube withdrawn, the patient being the whole time in the horizontal position. He continued perfectly insensible until between seven and eight o'clock the next morning, making altogether a period of sixteen hours. He recovered, and left the place the same day.

[It seems quite easy to empty the stomach when full enough by means of a syphon in this way, and we see no reason why the stomach may not as easily be *filled* as *emptied*. When to be filled, lift the tube *up* and pour the liquid in at the top;—when to be emptied, depress the end and make a syphon.]—*Brit. Med. Journal, May* 5, 1860, p. 341.

139.—ON THE UNIVERSITY OF ST. ANDREWS,

WITH REMARKS ON THE DEGREE OF M.D. OBTAINED THERE.

By WILLIAM BRAITHWAITE, M.D. (St. Andrews.)

"The University of St. Andrews has done good service in supporting this examination movement. We call attention to the papers of questions proposed to the Candidates, and feel assured that they would do credit to any body, however learned. It is one of the privileges of this country that she may feel proud of so many excellent schools of instruction."—*London Medical Review, Sept.* 1860, *p.* 109.

We were agreeably surprised during last September with the ancient University of St. Andrews. We confess that we were attracted to this seat of learning chiefly by the celebrity and high character of Dr. Day, its Professor of Medicine, and were not prepared to find that its importance as a seat of education had been so misunderstood and undervalued by English medical practitioners. Instead of the small and rather mean city, which we anticipated, we found that, by means of modern additions and improvements, it was not only rapidly advancing to be an enjoyable watering place, where people from various parts of Scotland were resorting for its healthiness, its splendid sands, its golf-ground two miles long, and its picturesque rocks and antiquities; but that it was also much frequented for the sake of its educational establishments. It will generally be found that in such places as Rugby, Harrow, &c., where there are good schools, families of moderate incomes will congregate for the sake of educating their boys in a superior manner, and at small cost. This seems to be the case at St. Andrews. It is rising up to be an important place of education in Scotland, and has, under the superintendence of Dr. Day, a superior University for the examination of medical practitioners and students for the degree of Doctor of Medicine.

By the late regulations of the different Medical Boards in Great Britain and Ireland, there are now, we believe, only two Universities where medical men, already in practice, can obtain the degree of Doctor of Medicine, without residing more or less at the place of examination. These two universities are the London and St. Andrews. The London University admits to examination students from different schools in England, but renders it very difficult, owing

to its stringent rules about the rudimentary knowledge which it requires from all its candidates, for medical practitioners of mature age and experience to obtain its degree. This, in our opinion, is a foolish and unnecessary restriction. Why compel a medical practitioner of 40, who is, perhaps, distinguished in his own town for character and success in practice, again to *rub up* the elements of knowledge, such as Greek and Latin, Anatomy, Physiology, Chemistry, and Materia Medica, which he knew well twenty years before, but which have now been succeeded by ripe experience in the knowledge and treatment of diseases? Why not examine such a candidate in his knowledge of Medicine, Surgery, and Midwifery, and on these subjects only be particular and exact, demanding that he shall be *well up* in every thing required at the bed-side. Now, this is just the practice at the University of St. Andrews, and hence its increasing popularity with all sensible men. At this University, a medical man of mature age and excellent character is examined most strictly in Medicine, Surgery, and Midwifery, including the Diseases of Women and Children; but those branches of knowledge which he is supposed to have known twenty years before, are considered in his case, of secondary importance. The younger candidates, indeed, are expected to be thoroughly conversant with *all* the subjects of study, especially such as young men *ought* to know; but in them, the higher branches of Medicine (such a practical knowledge of disease as can only be learned by years of experience) will, no doubt, hardly be expected, so that the candidates are strictly examined according to what is expected of them. The younger men being examined in Celsus, Anatomy, Physiology, Chemistry, &c., and being expected to have also a good knowledge of Medicine, Surgery, and Midwifery; while in the older candidates, this is very properly reversed. Hence St. Andrews is yearly becoming the Alma Mater of English practitioners, while London will continue to be only the resort of remarkably few candidates, instead of being, what it ought to be, the liberal Alma Mater of both the younger and the older practitioners of medicine in England. We would venture to recommend the London University to be as liberal and fair to men in practice, as is St. Andrews. Let the Examiners allow gentlemen of 40, who have

worked their way up in their respective towns and localities, often to a much higher social and professional position than their own graduates, to be examined *only* in Medicine, Surgery, and Midwifery; and let them insist on a deep knowledge of these practical subjects, and they will have a very superior class of men attempting to obtain the London Degree, which we consider now to be the best Medical Degree in the empire.

St. Andrews was one of the ancient cities of Scotland, the residence of an Archbishop for a great number of years, who possessed great power and wealth, and the ruins of whose Cathedral remain to this day a monument of his greatness. You enter the city in one direction by a gateway similar to what you find in other old towns, such as York. A new town of good houses and villas is gradually being added to the more ancient city. At the eastern extremity, on the abrupt rocky sea-shore, you come to the most interesting ruins of a large Cathedral, a very ancient square Tower of great height, and an old Castle of rather large dimensions, with its central court-yard, its rooms all round, and its *bottle-dungeon*, so called on account of its resemblance in shape to a bottle. These ancient edifices are built on commanding rocks, which must have a picturesque appearance from the sea. At a short distance are what are called the *Links*, but which in England would be called *Downs*, or fine grass land kept short by sheep feeding, and varied by sand-holes. These Links are about two miles long and constitute the play-ground of the inhabitants, both young and old, on which the Scotch game of *Golf* is nearly always going on. We never trod on firmer sands than at St. Andrews, extending for two or three miles, with a magnificent ocean foaming and roaring amongst the fine rocks which are there situated. In short, we found St. Andrews to be a place where the intellect could be gratified by one of the finest College Libraries in the Kingdom, where the artist could sketch the most interesting ruins, and where the athletic man or boy could play at golf to his heart's content. Besides these, you may *boat* and *fish* and *shoot ducks* for ever,—so that had it not been for a severe examination taking up most of our own time, we should have thoroughly enjoyed the place. We ought also

to say, that you have the most talented preachers, the worthy successors of John Knox, who often preached here, and whose pulpit is still preserved in one of the Colleges.

But it is with respect to the University of St. Andrews that we chiefly wish to enlighten the English reader, but not the Scotch reader, as he already knows and duly appreciates this seat of learning.

The University includes two Colleges, the United College of St. Leonard and St. Salvator now commonly termed the "United College," and St. Mary's College. In the United College there are Professorships of Latin, Greek, Mathematics, Natural Philosophy, Logic, Moral Philosophy, Civil History, Chemistry, and Medicine. This is the College of the Faculty of Arts: and after three years' attendance upon the prescribed course of classes, students can offer themselves for examination for the B.A. degree, a fourth year's study being required for the degree of M.A.* No student can commence his studies at the Theological College until he has attended lectures on Latin, Greek, Mathematics, Logic, Moral Philosophy, and Natural Philosophy, during four sessions. The session extends from the beginning of November to the end of April. St. Mary's College is devoted entirely to the study of Theology, and contains Chairs of Hebrew, Biblical Criticism, Church History, and Theology. In addition to the two Colleges which have been already mentioned, there is an establishment termed "The Madras College."

The Madras College was founded but a short time ago, by a bequest of about £60,000 by the late Dr. Bell, and is named the Madras College, because it was in Madras that *Dr. Bell himself* first tried the system, which is essentially the Monitorial one. It has nearly 1000 scholars, some commencing early and continuing there till 15 or 16, when they are ready, if they think fit, to finish their education at the United College, obtaining the usual degrees of B.A. and M.A.

* We may mention that the Latin, Greek and Mathematical classes are taught by gentlemen who have obtained the highest honours in their respective departments in the English Universities, and that the United College consequently affords an admirable education (on extremely moderate terms), for young men preparing for Oxford, Cambridge, and the India Civil Service, &c. The Professor of Medicine gives popular courses on Physiology and Comparative Anatomy.

The rest of the scholars, who may be intended for commerce or other industrial pursuits, do not enter College but go forth to all parts of Scotland to earn their livelihood as they best can: and this kind of education gives an intelligence to this class which we often find deficient in England.

We have only room here to dwell on that department of the University which has reference to Medicine. We may just say, however, that the Divinity College of St. Mary's is presided over by Dr. Tulloch, a man universally beloved and respected, not only for his great learning but for his urbanity towards every one; and that the Arts College, called the United College, is presided over by Professor Forbes, late of Edinburgh, whose name alone is a guarantee of every thing that is good; and that Dr. Day is in this College the Professor of Medicine—a man well known and appreciated by the whole medical profession, not only for his talent as the author of several original works, and as a writer in various medical publications, but for the zeal that he has exhibited, ever since his appointment, in endeavouring to raise the character of the St. Andrews degree. The Assistant Examiners have evidently been selected by the University with great care, as men whose reputation would add to the value of the degree conferred. For example, we have most of us heard of Dr. Alexander Wood and Dr. W. T. Gairdner of Edinburgh, of Dr. G. Buchanan and Dr. Struthers, of Dr. A. Anderson and Dr. J. M. Duncan. They are all men of note, and distinguished in their different departments for their acquirements and reputation. All these men must considerably add to the reputation of St. Andrew's, and make its degree rank high in medical and general estimation.

New regulations for students commencing their studies on or after Oct. 1, 1861, have lately been issued by the University of St. Andrew's, but these will not interest the great body of medical practitioners, and old students at the various medical schools, whom we are now addressing: and who will be examined according to the old regulations, a copy of which we here insert.

University of St. Andrews.

REGULATIONS OF THE SENATUS ACADEMICUS

RESPECTING THE

EDUCATION OF CANDIDATES FOR THE DEGREE OF DOCTOR OF MEDICINE.

I. Every Candidate for a Diploma in Medicine, upon presenting himself for examination, shall produce satisfactory evidence—

1. Of unexceptionable moral character.
2. Of having had a liberal and classical education.
3. Of having completed the 22nd year of his age.

II. Fellows, Members, and Licentiates of the Royal Colleges of Surgeons of England, Edinburgh, and Dublin—of the Royal College of Physicians of London—of the Faculty of Physicians and Surgeons of Glasgow—and of the London Apothecaries' Company—are eligible as Candidates for the Degree of Doctor of Medicine, on producing their Diploma or Licence.

N.B.—Notice to Students.—New regulations (copies of which may be obtained from the Secretary) have been published for Students commencing their attendance on Medical Classes in or after October 1861.

III. Candidates not holding any of the qualifications enumerated in the above clause, must produce satisfactory proof that they have regularly attended Lectures for four complete Winter and two Summer Sessions, on the following branches:—

1.	Anatomy	2	courses of six months each.
2.	Practical Anatomy or Dissections	12	months.
3.	Physiology	1	course of six months.
4.	Chemistry	1	do. do.
5.	Practical Chemistry	1	do. of three months.
6.	Botany	1	do. do.
7.	Natural History or Comparative Anatomy	1	do. do.
8.	Materia Medica and Pharmacy ...	1	do. do.
9.	Midwifery and Diseases of Women and Children	1	do. do.
10.	Medical Jurisprudence	1	do. do.
11.	Surgery	1	do. of six months.
12.	Clinical Surgery	1	do. do.
13.	Practice of Medicine	1	do. do.
14.	Clinical Medicine	1	do. do.

and that they have diligently attended Medical Hospital Practice for at least two entire years. The Lectures and Hospital Practice may be attended at any of the Medical Schools and Hospitals recognised by the Universities of London or Edinburgh, or by the College of Surgeons of England.

REGULATIONS RESPECTING THE EXAMINATIONS.

EXAMINERS FOR DEGREES IN MEDICINE.

GEORGE E. DAY, M.D., F.R.S., Professor of Anatomy and Medicine.
ARTHUR CONNELL, F.R.SS.L. and E., Professor of Chemistry (assisted by M. FOSTER HEDDLE, M.D.)

ASSISTANT EXAMINERS FOR THE YEAR 1861.

In Therapeutics and Practice of Medicine.

WILLIAM T. GAIRDNER, M.D., F.R.C.P., Lecturer on the Practice of Medicine, and on Clinical Medicine, Edinburgh.
ALEXANDER WOOD, M.D., President of the Royal College of Physicians, Edinburgh.

In Surgery.

GEORGE BUCHANAN, M.D., Surgeon to the Royal Infirmary, Glasgow.
JOHN STRUTHERS, M.D., Assistant-Surgeon to the Royal Infirmary, Edinburgh.

In Midwifery and the Diseases of Women and Children.

ANDREW ANDERSON, M.D., Professor of Medicine in the Andersonian University, Glasgow.
J. MATTHEWS DUNCAN, M.D., Lecturer on Midwifery, Edinburgh.

The examinations take place three times in the year, commencing on the first Monday in May; on the last Monday in September; and on Thursday, the 27th of December. The graduation fee is Twenty-five Guineas. In the event of a Candidate being found unqualified, he shall forfeit one-third of the graduation fee; which, however, will be accounted for to him when he passes his examination at a subsequent trial.

The examination by printed papers extends over three days, after which each Candidate is submitted to an oral examination.

All Candidates are required to give a written translation of a passage from the first four Books of Celsus, to write Prescriptions in Latin with accuracy, and to be so far acquainted with Greek as to be able to give the meanings of Scientific and Medical Terms derived from that language.

During the first three days of the examination, the Candidates answer printed questions on (1) Chemistry and Materia Medica; (2) Anatomy and Physiology; (3) The Practice of Medicine; (4) The Principles of Surgery; and (5) Midwifery and the Diseases of Women and Children. They are likewise required to write a short Commentary on a Medical, on a Surgical, and on a Midwifery case.

The Degree is conferred, at the conclusion of the oral examinations, by the Rector, in the Hall of the Public Library of the University, and the Diplomas are signed by the Professors of the University.

Every Candidate is required to present himself for Registration to the Secretary, on or before the Saturday preceding the May and September examinations, and on or before the day preceding the December examination, and to communicate by Letter with the Professor of Medicine, at least a fortnight previously, stating what Diploma or Certificates he intends to produce.

JAMES M'BEAN, A.M., *Sec.*

As the Examiners receive very frequent applications respecting the course of reading to be pursued, they beg to recommend the following works as especially deserving of perusal:—

Fownes's Manual of Chemistry. (Candidates who have been long engaged in practice are expected to possess, at least, a knowledge of the general principles of the Science, and an acquaintance with the ordinary Chemical Compounds used in Medicine.)

Christison's Dispensatory, or Pereira's Materia Medica (especially the sections treating of the mode of action, uses, and administration of Medicines), and the London, Edinburgh, or Dublin Pharmacopœia.

Gray's Anatomy, Quain's Elements of Anatomy, or Wilson's Anatomist's Vade-Mecum.

Carpenter's Principles of Human Physiology, or Kirkes' Handbook of Physiology.

Williams's Principles of Medicine, and either Watson's Lectures on the Principles and Practice of Physic, or Wood's Treatise on the Practice of Medicine (American)

Miller's Principles and Practice of Surgery, or Fergusson's System of Practical Surgery.

Ramsbotham's Principles and Practice of Obstetric Medicine, or Churchill on the Theory and Practice of Midwifery.

Candidates who have acquitted themselves creditably in the written examination are allowed to compete for Honours.

Candidates for Honours are additionally examined in Comparative Anatomy and Physiology, in the higher departments of Human Physiology and Pathology, and in Medical Jurisprudence; and their practical knowledge of Medicine is tested at the bedside.

ST. ANDREWS, *November* 1860.

All Medical Practitioners possessing the diploma of the College of Surgeons, or the Licence of the Hall, will come under the old regulations, and can at once proceed to St. Andrews. The best plan for Students at present, is to first obtain the diploma of the College of Surgeons and then to go to St. Andrews. They will then possess a

good degree and also the College diploma in surgery, thus fitting them for any office or position.

We will now proceed to explain what the examination is, and what is the best way for our professional brethren to pass it. The examination, last September, began on Monday morning. For two days we were occupied in a written examination. The hours were, in the morning of Monday, from 10 to 1 o'clock, and in the afternoon, from 3 to 5; on the Tuesday, from 10 to 12 in the morning, and from 2 to 5 in the afternoon. The two following days of Wednesday and Thursday were occupied by the different examiners in *vivâ voce* examinations of the respective candidates, each candidate going in according to his name standing alphabetically. On the Friday, the successful candidates had the degree of M.D. conferred on them, in the University library, by Dr. Tulloch, the Vice Chancellor, in presence of a goodly number of ladies and gentlemen of the city.

The following were the questions asked at this examination :—

UNIVERSITY OF ST. ANDREWS.

Medical Examination Papers.—September, 1860.

First Examination.—First Part.

To be translated into English.

Cognitis indiciis, quæ nos vel spe consolentur, vel metu terreant, ad curationes morborum transeundum est. Ex his quædam communes sunt, quædam propriæ: communes, quæ pluribus morbis opitulanter: propriæ, quæ singulis. Ante de communibus dicam: ex quibus tamen quædam non ægros solum, sed sanos quoque sustinent; quædam in adversa tantum valetudine adhibentur. Omne vero auxilium corporis, aut demit aliquam materiam, aut adjicit, aut evocat, aut reprimit, aut refrigerat, aut calefacit; simulque aut durat, aut mollit. Quædam non uno modo tantum, sed etiam duobus inter se non contrariis adjuvant. Demitur materia, sanguinis detractione, cucurbitula, dejectione, vomitu, frictione, gestatione, omnique exercitatione corporis, abstinentia, sudore. De quibus protinus dicam.

Give the origins or primary meanings of the following words: Astragalus, Blastema, Condyle, Cysticercus, Hemiplegia, Lithotrity, Peroneal, and Scaphoid.

Second Part.

Chemistry.—1. Water boils at 212° F. and freezes at 32° F. Is this statement universally true, or only true under certain conditions? What are the corresponding boiling and freezing points in the Centigrade Thermometer? Explain the method of converting degrees from one of these scales to the other. Reduce 100° F. to the Centigrade Scale and 256° C. to the Fahrenheit Scale. In the Arctic Regions would you prefer mercurial or spirit thermometers?

2. What compounds does oxygen form with carbon, hydrogen, sulphur, and phosphorus, and what are their respective formulæ?

3. Write down the formulæ expressing the composition of nitre, borax, alum, and corrosive sublimate, and explain the method of obtaining any two of these substances in a state of purity.

Third Part.

Materia Medica and Therapeutics.—1. Name the principal medicines which are commonly regarded as diuretics, and describe the modes in which they respectively act. Do these substances increase the aqueous portion of the solid constituents of the urine, or both? What effect upon the solid constituents of the urine is produced by copious water-drinking?

2. Are you acquainted with any substances which have the power of checking the general metamorphosis or disintegration of the tissues? If so, describe them; and explain their mode of action, and the cases in which their administration may be of service.

3. Quinine is sometimes adulterated with one or more of the following substances: gypsum, chalk, boracic acid, sugar, starch, and salicine. How would you detect these impurities?

Supposing that any other acid had been mixed with hydrocyanic acid, how would you detect the adulteration?

How would you determine the strength of any given specimen of hydrocyanic acid?

4. What is the preparation of bismuth that is commonly used in medicine? How is it prepared? What are its uses? Describe the symptoms which would lead you to prescribe it, and write a Latin prescription (without symbols or abbreviations) for a draught containing it.

Second Examination.

Anatomy and Physiology.—1. Describe the muscles which flex the leg upon the thigh.

2. What parts must be removed to expose the pterygoid muscles? Describe these muscles, noticing their relations to adjacent structures, the source from whence they derive their nerves, and their actions.

3. Describe the boundaries and contents of the axilla.

(N.B. Candidates who prefer answering this question fully may neglect the two preceding questions.)

4. What are the respective numbers of the temporary and the permanent teeth? State, as nearly as you can, the period at which you would expect

the different teeth to appear. Describe the structures which occur in the composition of a tooth. If you have studied comparative anatomy, you may describe the peculiarities of dentition in the ruminants and in the rodents.

5. Describe the appearance and structure of the villi, and explain the part which they are supposed to take in the process of intestinal absorption.

6. What are the different refracting media of the eye through which the rays of light must pass before they reach the retina? Explain how these different media modify the direction of the rays. What are the conditions giving rise to myopia and presbyopia? Explain the way in which glasses remedy these defects.

Third Examination.

N.B. In answering the practical questions, the examiners require every candidate to specify the mode of treatment which he is in the habit of adopting, and the doses of the medicines which he prescribes.

Medicine.—1. State what you know of the general doctrines of the hemorrhagic affections, especially as to their origin, causes, nature, and varities; and name the special hemorrhagic affections.

2. Describe the general principles applicable in the detection of valvular diseases of the heart, the diseases to which the valves of that organ are liable, and the means of detecting the particular valves affected, and the nature of the affection.

3. What advantages are gained by a selection of climate for invalids, what affections are likely to be benefited by such selection, and what climates would you select, 1st, for incipient phthisis—2nd, for chronic bronchitis, in this country and abroad?

4. What are the different forms of lithiasis, and what treatment would you adopt in each?

5. A robust young man was seized four days ago with headache and shivering. He went to bed and slept well all night, but on the following morning was seized with pain (severe and stabbing) below right nipple; his pulse then became rapid and skin hot. At the period of observation pulse 108, skin pungently hot, tongue covered with a thick white fur, rubbing sound with fine crepitation below right nipple.

What disease does the patient labour under? What would your prognosis be? How would you treat such a case?

6. A patient, aged fifty, is losing flesh and weight rapidly, subject to headache and palpitation; appetite voracious; urine plentiful, sp. grav. 1·040; sample of urine turned claret colour by liquor potassæ.

What is the disease, and how would you treat it?

Fourth Examination.

Surgery.—1. What is iritis? What are its symptoms and diagnostic marks, the results if it proceeds unchecked, and the treatment?

2. What is hydrocele? Mention its varieties. With what other tumours of the scrotum might it be confounded, and state how it may be distinguished from them?

3. Describe the various forms of the dislocation of the hip-joint.

4. Describe the process of formation of an aneurism. Mention the methods of treatment.

5. A man falls from a height on the top of his head, and is taken up insensible. On examination, no depression or other sign of fracture is found on the surface of the cranium. His breathing is laboured and his eye is insensible to light. He remains unconscious, and his urine and fæces are passed involuntarily. At first blood, then watery fluid flows from the ear. Ultimately he dies comatose.

What injury has been received? Explain how it has been produced, account for some of the most prominent symptoms.

What appearances will probably be found on post-mortem examination?

Fifth Examination.

Midwifery.—1. Mention the various circumstances which may make it proper to induce premature labour; and describe the different ways in which this may be effected, stating which you would prefer, and why so.

2. Recount the symptoms and signs of pregnancy, stating the circumstances which may in particular cases obscure the diagnosis.

3. What are the points in the history, the symptoms, and the physical signs of a case, which enable us to distinguish ovarian dropsy from ascites?

4. A woman, on the day after a very severe labour, in which turning had to be performed, had a rigor, which returned from time to time during the next week, being always followed by perspiration, and accompanied by gradually increasing asthenic fever. Her pulse was extremely rapid and feeble, her tongue dry and brown, her skin of a dusky yellowish colour; the lochia were fœtid, and the milk suppressed. She became very restless and sleepless, and died before the end of the fortnight, the region of the uterus having been only slightly tender on pressure. What was the nature of her disease, what the morbid appearances to be expected in the dead body, and what treatment ought to have been employed?

Now this is the examination which we underwent last September, and we maintain that a better set of questions could hardly be asked by any examining board in the kingdom; and we will also venture to add that we believe, from the conversations which we had with several of the candidates, these questions were in most instances well answered. The writer of this pamphlet can only say for himself that it occupied him the best part of two days to do justice to these questions, and to answer some of them he had to *ransack* his old brains severely for some hours.

In short, we consider that the examination at St. Andrews is one of the best in the kingdom, and one of the fairest too: not exacting too much of that knowledge from young candidates which can only be obtained by years of experience at the bed-side, nor too much of that elementary knowledge from the older candidates which they knew in bygone days but have now forgotten. Every candidate has fair play. If his written answers are comparatively defective he is examined with more strictness orally, while if they are quite satisfactory to the examiners, he has little to dread from his oral examination.

Let us now recommend all our professional brethren to go to St. Andrews without loss of time. It is just possible that under some future new regulations the Commissioners for the Scotch Universities might insist on residence at St. Andrews, as they have done at other Universities, although we think that this would be a great injustice to St. Andrews, seeing that there is no hospital, and considering that it almost strictly resembles the London University, both being chiefly examining boards, although St. Andrews has the advantage of colleges attached to it. But, for medical students, what is a college without an hospital? If the Commissioners close St. Andrews to all but resident candidates, we can only say that practitioners will apply to Foreign Universities for degrees, as has been the case already to a disgraceful extent, or perhaps to the Archbishop of Canterbury, whose degrees (called Lambeth degrees) have now become a laughing stock to the medical profession, on account of their profusion amongst the ranks of Homœopathy, and obtained apparently simply by purchase, without that examination which confers honour on those who undergo it. What a pity that so high a dignitary of the English Church should allow such disgrace to fall on his honorary degrees, that they are despised by all respectable medical practitioners who know anything about the matter!

In preparing for this examination we would recommend the younger candidates to be *well up* in all the books mentioned at page 426. To the older practitioners we would say,—go up *well read* in Physiology, Medicine, Surgery, and Midwifery, especially in the books recommended: and in Celsus, Chemistry, Materia Medica, Anatomy, it will

be sufficient to be *pretty well up*. It would be unreasonable to expect a man at 40, who had been hard at work in practice for perhaps fifteen or twenty years, to get up his Chemistry, Materia Medica, and Anatomy again; and we know that this is not expected of him: what is expected of such a candidate is, that he should be *well up* in every thing belonging to *Bed-side practice*,—the use of the stethoscope, tests for urine, Toxicology, the diagnosis and treatment of diseases of the Nervous System, Heart, Liver, Kidney, Lungs, &c., and such other diseases as every good Physician ought to know. He should also be well up in the Principles of Surgery, in the Practice of Midwifery, and the diseases of Women and Children. If he be well informed on these practical subjects he need not fear being rejected at St. Andrews. It is the most *sensible* and fair examination of any of our Universities, and must rank high with every one who knows anything about the matter.

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